THE

PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY,

AGRICULTURE,

MANUFACTURES AND COMMERCE.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

VOL. XLIX.

For JANUARY, FEBRUARY, MARCH, APRIL, MAY, and JUNE, 1817.

LONDON:
PRINTED BY RICHARD AND ARTHUR TAYLOR, SHOE LANE:
And sold by CADELL and DAVIES; LONGMAN, HURST, REES, ORME, and BROWN; MURRAY; HIGHLEY; SHERWOOD and Co.; HARDING; UNDERWOOD, London: CONSTABLE and Co. Edinburgh: BRASH and REID; DUNCAN; and PENMAN, Glasgow: and GILBERT and HODGES, Dublin.

CONTENTS

OF THE FORTY-NINTH VOLUME.

CONTENTS.

CONTENTS.

CONTENTS.

THE

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

I. *On Terrestrial Magnetism.*

To Mr. Tilloch.

Sir, — It happens but seldom that a single chapter of a treatise on any subject is so completely disengaged from the rest of the work as to allow of its being published separately. There are usually so many references, and dependences of one part on another, that it will not admit of being detached. The following, however, is an exception. It forms the tenth chapter of the third volume of the Treatise on Experimental and Mathematical Philosophy, lately published at Paris by M. Biot of the Institute &c.: a work of very distinguished merit, having a considerable portion of its contents treated in a manner altogether new, and containing deductions from the experiments of a most interesting and important nature.

The study of terrestrial magnetism has latterly declined very much in this country; which is readily accounted for by the uncertainty attending most of its conclusions. It excited great interest, and indeed was one of the principal objects of pursuit, in England, about the end of the seventeenth and the commencement of the eighteenth century.

Amongst a number of persons who have exerted themselves in this department of knowledge we are particularly indebted to Dr. Halley, who recommended the matter to the earnest attention of Government, and after unwearied solicitation obtained permission for a ship to be fitted out and sent with him on a voyage for the purpose of making magnetical observations. In this he repeatedly traversed the Atlantic Ocean, and enriched various parts of nautical science with the fruits of his labour, as is abundantly testified by the numerous papers which he afterwards published

in the Philosophical Transactions, the *Miscellanea Curiosa,* and other works. On his return from this expedition, he combined his own observations with those of others, and from them constructed a chart, with the curve lines of the various degrees of variation of the compass drawn upon it, in directions corresponding with those on the surface of the earth. The idea of this chart was at that time perfectly new: and men of science were led to imagine that the longitude might be discovered by means of it with sufficient accuracy for nautical purposes. Were these lines permanent, or the changes which they undergo reducible to any calculable laws, it might be determined by them within certain limits; but this is not the case: and although the accompanying paper elucidates several important points, and brings before us the active labours of many ingenious individuals; yet it has not been successful in removing all the difficulties that present themselves in its application to the longitude.

It is well known to philosophers, that there was a time when the laws of the moon's motions were considered so inexplicable as to defy all human attempts to reduce them to any simple principles, capable of being applied for predicting her future situation for any given time with exactness: and the same could then be said of the tides, the orbits of comets, and various other particular parts of astronomy and natural philosophy: but by the unwearied diligence and researches of a few profound mathematicians, this uncertainty respecting them now no longer exists. There are, indeed, few things of this kind that cannot by degrees be brought to some system. Empiric modes are first applied of explaining and computing the several motions; then by investigating, comparing, and gradually approximating to the observations, we come at length to causes which rest on established principles, and ultimately every apparent anomaly is accounted for, by a reasonable and satisfactory theory.

Mr. Ralph Walker, who published a treatise on this subject in 1794, has computed a table* extending from the equator to the latitude of 60° north, and from the meridian of London to 90° of west longitude, in which, by entering with the latitude on the side, and the longitude at the top, the corresponding variation of the compass may be taken out of the area of the table; or, by means of the variation, and the latitude of the place, the longitude may be obtained†. But such is the imperfect state of

* A similar one is given by Captain Christopher Middleton, to suit the voyage from London to Hudson's Bay, in the Philosophical Transactions, No. 393, and adapted for the year 1725.

† It is evident, that such a table must be useful in more respects than the one mentioned here; for it enables seamen, who know their situation in

of our knowledge of the changes that occur in the variation, that the situation of the lines before mentioned on the chart cannot be predicted for any distant future period: and consequently the method at present fails. By combining the *dip* with the *variation*, a much greater degree of certainty will be given to it when the laws of both are better understood: and perhaps it will be still further improved by adding the *intensity*; or the number of vibrations which each of these needles makes in a given time: a property that was first noticed by our ingenious countryman Mr. George Graham, in the Philosophical Transactions for 1725, p. 332. At present, however, little or nothing can be done to forward the purpose in question, for want of sufficient observations and data, on which to establish a permanent theory.

It will probably be asked, Why should we take pains to bring this method to perfection, which at best can only be an approximation, when we possess so many others that are founded on principles not liable to uncertainty? The answer is plain and cogent: It ought on no account to be neglected, whilst there exists the least probability of its ever being applied to any useful purpose, or whilst a single property of so curious a subject is not thoroughly understood. By attempting to prove the existence of an annual parallax of the earth, the aberration of light and the nutation of the earth's axis were discovered: and is it possible to foretell that nothing will be found out by endeavouring to render magnetism of use to seamen? Perhaps it may be found to form some connecting link in the chain of unexplained processes that are always going forward in the formation and decomposition of mineralogic substances in the interior of the earth; or perhaps it may account for certain actions in the arrangement of their particles, especially when ferruginous matter enters into the combination. The cultivation of any department of knowledge affords pleasure: but to examine the operations of nature, to reduce them to system, and to bring her laws to light, is certainly the most exalted exercise which the human mind, in its present imperfect state, is capable of enjoying. If the longitude cannot be found by it, there will be a gratification in having the cause of its failure satisfactorily explained. Till then it cannot be relinquished without regret. Perhaps even the attempt to show its impracticability may elicit some new idea, or lead us to combine it with some extraneous principle or science, which may render it effectual for the pur-

in the Atlantic Ocean within a trifle by the dead reckoning, to proceed along the shortest and most expeditious track by informing them what variation they are to allow in the courses which they ought to steer.

 pose.

pose. Attempts to square the circle have been made without number; and persons might longer have continued to waste time and talents in this fruitless inquiry, had not Lambert at length demonstrated its impossibility in a rational form*.

In the following Memoir a general view is given, by its ingenious author, of all the most material properties that are known on the subject of terrestrial magnetism. Some new and curious mathematical results respecting the inflexions of the magnetic equator, and the lines of no variation and dip, are derived from the observations made in different parts of the world; and an hypothesis is proposed for explaining them, which, if not quite correct and satisfactory, at least represents the magnetic actions, and their modifications, in a way that cannot easily be controverted, until further discoveries shall have made us better acquainted with their causes.

It is earnestly wished that astronomers, travellers, captains of ships, and other gentlemen who possess the necessary instruments, would continue these researches, and publish their observations; for it is only by discussing a series of them, made in a great number of places, and continued for a long period of time, that we can expect to arrive at a complete knowledge of the laws of magnetic attraction over the whole surface of the earth.

Particular requests might be given to the commanders of vessels in the service of His Majesty, the East India and other public and private companies, to determine these data as often as the weather and circumstances will admit. Not only should the dip and variation be taken, but the number of oscillations also of both these needles in ten minutes, commencing at a certain distance from zero, as was begun by Mr. George Graham, and continued by Humboldt and Rössel. It has been proved by the experiments of Mr. Canton, that the temperature of the atmosphere has a sensible influence on the variation of a needle; and he accounts for the diurnal alterations by means of it; but whether the density, or any of the other modifications of the atmosphere, besides aurora borealis, and lightning, will alter its direction, cannot in the present state of our knowledge be ascertained: it will therefore be proper to note down the barometer and thermometer, at least, with the latitude and longitude of the place of observation at the time of making each experiment. Considerable difficulty will be met with occasionally in taking them at sea, on account of the action of the iron which there is in the ship, the instability of the water, and other causes: but perhaps the method now generally adopted, of sus-

* Memoirs of the Royal Academy of Sciences of Berlin for 1761.

pending

pending marine barometers in gimbals, just above the centre of gravity, may, under certain modifications, be applied with advantage to these compasses, in keeping them steady when the sea is rough. The mean of a number of observations will give the dip and variation with tolerable exactness, even when little dependence can be placed on a single attempt. They can always be taken on land, with all the accuracy that the nature of the subject admits, provided the ship remain at anchor a sufficient time for the purpose.

Exclusive of any view to utility, the subject, like all the other operations of nature, is in itself extremely curious; and, it is presumed, will amply repay, with interesting and amusing novelties, whatever trouble may attend its cultivation. With so great a probability of its becoming useful in the important art of conducting ships from one part of the world to another, or of its leading to useful discoveries in the œconomy of nature, by explaining the causes of important changes in the mineralogic and geologic departments, it cannot but recommend itself strongly to the attention of all those who feel pleasure in the advancement of useful scientific knowledge.

I remain, sir,

Yours, &c.

Christ's Hospital, Jan. 1, 1817. T. S. Evans.

On the Laws of Terrestrial Magnetism in different Latitudes.

We have mentioned elsewhere, that the *dip*, the *variation*, and the *intensity* of the magnetic force, are not the same in all parts of the earth. We now possess all the necessary processes for giving the present state of these phænomena with exactness. It is only requisite to carry with us to different places the same magnetic needle, or others that are capable of being compared together, and to observe the three elements or data above named in those places.

This great experiment was undertaken about the year 1700, by the celebrated astronomer Dr. Halley, to whom the English Government gave the command of a vessel for the purpose of carrying him and his instruments to different parts of the globe. But the researches of Dr. Halley having for their object the determination of the longitude of places by means of the variation of the compass, he confined himself principally to observe this element, which unfortunately appears to be the most variable of the three. So that, if we wish to describe the present state of terrestrial magnetism, we are obliged to

A 4 have

have recourse to the detached observations of more modern navigators. But the needles which they have made use of not admitting of comparison one with another, and their methods of observing not being the same, it is conceived that these differences must produce many apparent anomalies in the results; and, therefore, we can at most only hope to find out the general circumstances of these phænomena, without being able to estimate them in detail. Lastly, what increases these difficulties still further is, that we are wholly in want of observations, made in a great part of the globe, where they are so much the more necessary, as the whole of the facts appear to indicate there the action of some extremely remarkable local causes, of which it is impossible to form an idea, except from the experiments themselves. For this reason, I must here confine myself to point out merely what is at present known of the general facts of these phænomena, without undertaking to connect them by calculations, for which the most essential data are wanting. This will be sufficient to inform navigators of the places on the globe where it would be most useful for them to direct their attention and to increase their observations.

I shall first consider the inequalities of the magnetic *dip* in different climates of the earth; because this phænomenon appears to vary with time, much less than the variation. The first thing to be done, in order to discover some of its laws, is to determine the points on the globe where the dip is nothing; or where a needle, that is perfectly horizontal before being magnetized, still keeps the same position afterwards. The series of these points form a curve line on the surface of the earth, which is called *the magnetic equator*, and which all authors have hitherto considered as a great circle, inclined in an angle of about twelve degrees to the terrestrial equator. This, indeed, is what is indicated by all the observations made on an extent of more than 180° of longitude in the Atlantic Ocean, the Indian Sea, and that part of the South Sea which washes the coasts of South America. To explain this, let $M' M''$ (Plate I. fig. 1) be two points on the globe, where observations have shown that the dip is nothing; draw the great circle $A E' E''$ to represent the terrestrial equator, and let AM be another great circle perpendicular to $A E' E''$, representing the terrestrial meridian, of which the longitude is reckoned on the equator. Then, if from the places of observation $M' M''$ we draw other portions of meridians $M' E'$, $M'' E''$ terminated also in the equator, the arcs AE', AE'' which I call l' and l'', are the longitudes of the places $M' M''$, and the arcs $E' M'$, $E'' M''$, which I call λ' and λ'', are their geographic latitudes. This being done, if by these points we draw an arc

$M'' M' N$

M″ M′ N′ of a great circle, which when produced cuts the equator somewhere in N′, at a distance from the point A, equal to A N′ or x, and under an inclination M′ N′ E′, which I designate by i; then the two spheric triangles M′ N′ E′, M″ N′ E″ right angled at E′ and E″, give these two equations,

$\text{tang } i = \dfrac{\text{tang } \lambda'}{\sin(l'-n)}$; and $\text{tang } i = \dfrac{\text{tang } \lambda''}{\sin(l''-n)}$, from which we obtain $\text{tang } x = \dfrac{\text{tang } \lambda'' \sin l' - \text{tang } \lambda' \sin l''}{\text{tang } \lambda'' \cos l' - \text{tang } \lambda' \cos l''}$.

This equation will determine x; that is the longitude of the node of the great circle from any fixed meridian A M, and the other will determine M′ N′ E′, or the inclination of the magnetic equator* to the terrestrial equator. Now, if all the observations made in different parts of the world, when combined two and two, give nearly the same values always for x and i, we may conclude that the magnetic equator is a great circle of the terrestrial globe, at least in the extent embraced by the observations which we have used. To show how far this indication is satisfactory, I have computed the following table:

Place of Observation.	Name of the Observer.	Date.	Latitudes.		Longitude from the Meridian of Paris.	Inclination of the great Circle.	Longitude West of the Node.
			° ′ ″		° ′ ″	° ′ ″	° ′ ″
Atlantic	La Perouse	1786	11 9 7	S.	24 17 10 west	11 9 7	114 22 54
South Sea	La Perouse	1786	0 50 30	N.	118 39 20 west		
IndianSea	Bayly	1780	7 51 57	N.	103 53 32 east	12 51 45	113 20 19
Atlantic	Bayly	1780	12 48 36	S.	18 4 47 west		
Atlantic	Lacaille	1750	11 30 0	S.	10 30 0 west	12 3 32	118 15 25
Atlantic	Lacaille	1750	12 0 0	S.	34 0 0 west		
Atlantic	La Perouse	1786	11 9 7	S.	24 17 10 west	11 11 32	119 13 15
Peru	Humboldt	——	7 1 49	S.	80 39 59 west		
South Sea	La Perouse	1786	7 1 53	S.	118 55 30 west	12 33 22	114 17 19
Peru	Humboldt	——	7 1 49	S.	80 39 59 west		
IndianSea	Bayly	1780	7 51 57	N.	103 53 32 east	12 40 30	113 55 43
Peru	Humboldt	——	7 1 49	S.	80 39 59 west		
					Mean	12 5 0	115 34 0

* This expression for the *tang x* is not very convenient for computing by logarithms; it would be better thus:

$$\text{tang}(l'-x) = \frac{\text{tang } \lambda' \sin(l''-l')}{\text{tang } \lambda'' - \text{tang } \lambda' \cos(l''-l')};$$

after which, taking an auxiliary angle φ, such that

$$\text{tang } \varphi = \frac{\text{tang } \lambda' \sin(l''-l')}{\text{tang } \lambda''};$$

we shall find by eliminating tang λ'',

$$\text{tang}(l'-x) = \frac{\sin(l''-l')\sin\varphi}{\sin(l''-l'-\varphi)}.$$

The above table has been computed by these formulæ.

The

The agreement of these results is certainly very remarkable. We appear to be well authorized in concluding that the magnetic equator is really a great circle of the terrestrial sphere, inclined to the equator in an angle of about 12°, and having its western node situated at about 115° 34′ of west longitude from Paris*; that is, in the South Sea a few degrees from the island of Gallego, at nine hundred leagues from the coast of Peru. This places the opposite node in 295° 34′ of west longitude. Such indeed has hitherto been the general opinion. But what is extraordinary, these elements are wholly defective, in all that part of the South Sea, situated below the western node between 115° and 270° of longitude, which comprehends nearly a whole hemisphere of sea. Indeed, in discussing some observations made with the utmost care by Captain Cook and Mr. Bayly, in two different vessels which sailed together in the South Sea, in 1777, I find that they have *both of them* met with the magnetic equator in 158° 50′ 9″ of west longitude, and in 3° 13′ 40″ of south latitude; whereas, by prolonging the great circle, that our first observations have given us, this equator ought then to have been found in north latitude 8° 36′ 30″. This shows us, therefore, that the magnetic equator, after having crossed the terrestrial equator, at about 115° of west longitude, descends again into the southern part of the globe: and as the observations of Mr. Bayly which are confirmed by those of Mr. Dalrymple, show likewise that this line is without dip in about 7° of north latitude in the Chinese sea, at 256° of west longitude, we must conclude, that between this longitude and that of 158° 50′ determined by the observations of Captain Cook, the magnetic equator and the terrestrial equator have at least one more intersection, independently of the eastern node, situated in the Indian Sea, at about 295° of longitude, and dependent on the circular part. There are therefore in all at least *three* nodes, and perhaps *four*, if the magnetic equator, near its western node, rise a little towards the north, before it descends again into the south, near the archipelago of the Society Islands. Fig. 2, represents the series of these inflexions, of which we shall presently find some striking confirmations in the effects we shall derive.

In examining the magnetic dips observed on both sides the line just traced, we find that they increase in proportion as they recede from it. If we confine ourselves to consider that half of the globe where the magnetic equator appears to be exactly circular, and which comprehends Europe, Africa, the Atlantic Ocean, and the eastern coasts of the two Americas, we find that

* Paris is 9′ 21″ of time to the eastward of Greenwich, or 2° 20′ 15″ of space; consequently this node is in 113° 14′ of west longitude from the Royal Observatory of Greenwich.

the

the dip remains nearly constant on the parallels situated at equal distances on both sides this equator: so that, by following this law, the maximum of dip takes place in two opposite points of the earth, of which the one situated towards the north will be found in about 25 degrees of west longitude, and (90° − 12° or) 78° of north latitude; whilst the other, diametrically opposite to it, will have 205° of west longitude, and 78° of south latitude.

Here, therefore, are the *poles* of the magnetic equator; and such indeed are approximately the positions that Philosophers have before assigned to them*. But if we confine ourselves to the consideration of that half of the earth where the laws of the dip appear to be the most simple, we may proceed far beyond these general indications. For we can represent these dips within a trifle in numbers, by supposing a very small magnet at the centre of the earth; or, which is more accurate, two magnetic centres, infinitely near each other, whose actions are exerted on all points of the surface of the globe, according to the common laws of magnetic forces; that is to say, in the inverse ratio of the squares of the distances. This result is confirmed by the observations given in a Memoir published by M. Humboldt and myself on the alterations of terrestrial magnetism in different latitudes. If we call i the dip of the magnetic needle, towards the north, in a place whose magnetic latitude is λ', we have

$$\text{tang}\ (i+\lambda)=\frac{\sin 2\lambda'}{\cos 2\lambda'-\frac{1}{3}}.$$

* Mr. Ralph Walker placed the south magnetic pole in latitude 65°, and longitude 130° east; and the north pole in latitude 71° and longitude 80° west, from observations which he says he made "in July last," and his book was published in 1794.

Mr. Leonard Euler, in an elegant dissertation on this subject, published in the Memoirs of the Academy of Sciences of Berlin, placed the north pole in 76° of north latitude and 96° west from Teneriff, and the south pole in 58° south, and 158° west from Teneriff. These are for the year 1757.

Mr. Churchman supposed, in 1800, the north pole to lie in latitude 58° north, and longitude 134° west from Greenwich, near Cape Fairweather; and the south pole in latitude 58° south and longitude 165° east from Greenwich. He also imagined that the north pole has moved to the eastward on a parallel of latitude about 65° since the beginning of 1600, and concludes that it makes a revolution in 1096 years. The south pole has moved less, and completes its revolution in 2289 years.

Krafft placed the north pole in latitude 70° north and longitude 23° west from London, and the south pole in latitude 50° south and longitude 92° east.

Wilcke placed the north pole in latitude 75° north, and in the longitude of California, and the south pole in latitude 70° south in the Pacific Ocean.

This

This formula requires that we know how to calculate λ'. Let A E be the terrestrial equator (fig. 3); N E′ the magnetic equator (supposed in like manner to be a great circle), and M the given place on the globe, having for its longitude $AE = l$, and for its geographic latitude $ME = \lambda$. If from this point we draw the arc of a great circle M E′ perpendicular to the magnetic equator, this arc will express the magnetic latitude of M. Now as we know the longitude AN, or a, of the node of the magnetic equator; expressing it by a, we shall have $NE = l - a$. Thus in the spheric triangle MNE, right angled at E, we know the two sides NE, ME: we may therefore compute the hypothenuse MN, or H, and the angle N, by these formulæ:

$$\cos H = \cos \lambda \cos (l - a) \text{ and } \tan N = \frac{\tan \lambda}{\sin (l - a)}.$$

The angle N being thus known, we add to it the angle of inclination I of the two equators, and we get the angle MNE′. Then in the triangle MNE′, the arc ME′, or λ' = the magnetic latitude of the point M will be obtained by the formula

$$\sin \lambda' = \sin H . \sin (N + I).$$

Now let us compute these for Paris.

Here the longitude $l = o$; the latitude $\lambda = 48° \, 50' \, 14''$; NE or $l - a$ will be $64° \, 26'$; that is to say, equal to the longitude of the eastern node of the magnetic equator. With these data we find

$$H = 73° \, 29' \, 10''; \; N = 51° \, 44' \, 10''; \text{ and } \lambda' = 59° \, 20' \, 10''.$$

Lastly, with this value of λ', computing $i + \lambda'$, and i, we find

$$i + \lambda' = 132° \, 49' \, 20''; \text{ and consequently } i = 73° \, 29' \, 10''.$$

This is therefore the dip of the magnetic needle at Paris, according to our formula: direct experiments give it about 70°.

Our formula, therefore, gives a very simple relation between the observed dips near the magnetic equator. Indeed, in this case i and λ are very small quantities. By confining ourselves to their first powers, we may consider $\cos 2\lambda$ as equal to 1; and we may substitute for $\tan (i + \lambda')$, and for $\sin 2\lambda$, the arcs which correspond to them. Then the formula is reduced to

$$i = 2\lambda';$$

that is to say, *each dip is double the corresponding magnetic latitude*. This property is found to be completely substantiated in all the observations made *at a little distance from the magnetic equator*, between the longitudinal limits where it is apparently circular.

For example: At Tompenda in Peru, M. Humboldt observed the

the dip to be 3° 11′ 42″, which gives λ' or the magnetic latitude of Tompenda equal to 1° 35′ 36″. By calculating it from its geographic position, we obtain 1° 28′ 55″.

Another example: At Quito in Peru, the magnetic latitude computed according to the geographic position is $\lambda' =$ 6° 33′ 10″

Doubling it we get the dip .. .. $i = 13$ 6 20

Humboldt found it by observation .. $i = 13$ 21 54

The observations of La Perouse and Lacaille near the magnetic equator, in the Atlantic Ocean and the Indian Sea, being reduced in the same manner, present us with results which agree in a similar way with the formula. Unfortunately these simple laws do not extend to the opposite side of the globe, which is affected by the inflexions of the magnetic equator. If we attempt to apply the exact formula to some one of the southern islands of the South Sea,—to Otaheite, for example, where Captain Cook so often observed,—we find the southern dips far too great; and on the contrary, in countries situated in North America, nearly in the same longitude, the computed dips are much too small. These deviations result necessarily from the inflexion, which in this part of the globe brings the magnetic equator towards the south pole; and they give a striking confirmation of it.

To agree with these phænomena, it is therefore necessary to suppose some perturbating local cause to exist near the archipelago of the South Sea, such as a particular centre of magnetic forces, which influences them in this hemisphere and modifies the central action. Indeed, by adopting this supposition it makes all the results agree: and they only require a very weak force in the secondary centre, which derives almost the whole of its energy from its proximity. But before we try to define and measure it, we must consider attentively the alterations that the variation of the needle and the intensity of the magnetic forces experience in different latitudes. For these phænomena, being also the results of the magnetic action of the earth, must be taken into consideration, if we would account for them completely.

To understand the dips, we began by seeking the series of places where they are nothing. In the same way, to discuss the phænomena of the variations, we must begin by finding the points on the globe where they also are nothing. The series of these points form what are called *lines of no variation**. These lines do not follow the direction of the geographic meridians on the globe, but are very oblique to them; and they present us

* The necessary elements for this discussion were furnished by M. de Humboldt.

with

with some very irregular inflexions. According to the most recent observations, there exists at present a line of no variation in the Atlantic Ocean between the old and the new world. It cuts the meridian of Paris, near the south latitude of 65°; from this it proceeds in a NNW direction towards about 35° of west longitude, whence it reaches as high up as the coasts of Paraguay; then, after going on nearly north and south, it runs along the coasts of Brasil, and thus proceeds to the latitude of Cayenne: but then turning suddenly to the NW, it directs its course towards the United States, and thence towards the other northern parts of the continent of America, which it traverses in the same direction.

The position of this line of no variation is not fixed on the globe; at least it has not been so during the last century and a half; but has moved considerably from the east towards the west. It passed through London in 1657, and through Paris in 1664*. So that, according to its present direction, it has described about 80° of longitude in 150 years along this parallel. But there is no doubt that this alteration is not uniform in its motion: it is, indeed, very unequal in different parallels; at Jamaica, for example, the variation has not experienced any sensible change for 140 years. In general, according to the present slowness of this motion, we are not certain that it is always progressive, nor that it proceeds in any given direction. These are things that time alone can bring to light.

Some very accurate observations of *the dip*, made at different times by the Honourable Mr. Cavendish and Mr. Gilpin in London, have proved that this element is likewise variable, although much less so than the variation. In 1775, the dip in London was 72° 30′: in 1805, it was 70° 21′. This has also been confirmed in France by the experiments of M. Humboldt.

There exists another band having no variation nearly opposite the preceding one: this runs in a continued direction to the NW, takes its commencement in the great South Sea, cuts the western point of New Holland, traverses the Indian Sea, enters the continent of Asia at Cape Comorin, and thence passing across Persia and Siberia westward, rises up towards Lapland. But what is very remarkable, this line bifurcates near the great archipelago of Asia, and gives rise to another branch, which, directing itself almost wholly from south to north, passes this archipelago, crosses China, and returns into itself again in the eastern part of Siberia. The existence of this branch, and its separation from the preceding one, are clearly pointed out by observations made in the Chinese seas. But I am enabled to

* See Walker's Treatise on Magnetism, p. 197.

present

present a still more certain confirmation of it in the variations observed in Russia, and on the frontiers of China, by the celebrated astronomer Schubert, who has been so kind as to communicate them to me. For, having gone in the summer of 1805 from Casan to Tobolsk, and from Tobolsk to Irkutsk, he met the two branches we have just described, one after another in their northern parts, where they are widest asunder. This is clearly pointed out by his observations, which I feel it my duty to record here.

TABLE.

Place.	Latitude North.			Longitude West from Paris, in Time.			Eastern Variation.		Dip.
	°	′	″	h	′	″	°	′	
Casan..........	55	47	51	3	3	38	2	2½	
Pekin..........	58	1	13	3	31	46	1	1	
Catherineburg ..	56	50	43	3	48	57	5	40	
Tobolsk........	58	11	56	4	18	30	7	9	78°
Tara..........	56	54	46	4	42	27	6	6	
Touisk........	56	*	42	5	30	0	5	37	
Nish-Udinsk	54	55	21	6	26	46	2	40	
Irkutsk.........	52	16	44	6	47	25	0	32	67°

The two branches which compose this line either never move, or move very slowly. It is certain that the variation has not altered at New Holland during the last 140 years.

Some indications are also to be found of a fourth line, without any variation. It was observed by Captain Cook in the South Sea, near the point of the greatest inflexion of the magnetic equator. This line has not been discovered by navigators further towards the north; but it is extremely probable that it is continued there: for, according to a very just remark made by M. Humboldt, since on both sides of each line of no variation, the variation changes its sign from east to west, it necessarily follows, that on the whole surface of the globe the number of lines without variation must be *even*, that it may fall again on the same sign, after all the alternations from *plus* to *minus* †.

Having

* The minutes were destroyed by the seal of the letter.

† This will be easily understood by inspecting fig. 5. The circle A is divided into *three* parts: and if we mark the first arc CD with the sign + ; the second DE with — ; and the third EC with + ; the first and last arcs will have the same sign, which is not according to nature. On the contrary, in the circle B, which is divided in *four* parts, if we mark the first FG with + ; the second with — ; the third with + ; and the fourth with — ; there will be a regular alternation of the signs in proceeding round the

Having determined the direction of the lines without variation, it is necessary to fix the other limits of the phænomena, that is, to trace the series of places where *the variation is greatest.* In this we find lines quite as irregular, which interpose themselves between the former. The greatest of all the variations that have been observed in the southern hemisphere, was by Captain Cook in latitude 60° 49′, and in 93° 45′ of west longitude, reckoned from the meridian of Paris; it was 43° 45′. The greatest of all those that have been observed in the northern hemisphere was also by Captain Cook, in 70° 19′ of latitude, and 161° 1′ of east longitude; it was 36° 19′ east.

[To be continued.]

II. *Remarks and Suggestions, for improving the British* COIN, *and the keeping of Money Accounts, preparatory to the entire Introduction of Decimal Money. By Mr.* JOHN FAREY *Sen.*

To Mr. Tilloch.

SIR, — THE great importance of simplifying our Coins, and money transactions and Accounts, by assimilating all of these with the *decimal* notation of our arithmetic, will I trust plead my excuse, for transcribing and sending for insertion in your work, two Letters and a Paper of suggestions, which, during the parliamentary discussions last summer, as to a new coinage, I addressed to a member of His Majesty's Privy Council, to whom science and the useful arts, are under no ordinary obligations.

I have nothing further to add, except to inform the purchasers of the two former volumes of my Derbyshire Report, herein referred to, that I am utterly unacquainted with the reasons which, since June last, have delayed the appointment of a Publisher and the sale of the 3d and concluding volume, and unable to say, when they may complete their sets of the work, of

Sir,

Your obedient humble servant,

Howland-street, Jan. 1, 1817. JOHN FAREY Sen.

" 37, Howland-street, Fitzroy-square, June 14, 1816.

" SIR,—The subject of our national Money appears to me a matter of such *very great importance*, that although at this time

the figure. Now this will always be the case when the number of divisions of the circle is *even*; but it will be otherwise when the number of them is *odd*. It is therefore extremely probable, that there are on the surface of the earth an even number of lines of no variation, as Humboldt has remarked.

exceedingly

exceedingly engaged, in preparing to leave town, I have taken the liberty of drawing up a Paper of suggestions, and inclosing two copies to you, for explaining the very simple, easy and useful improvement that might be made, in our *small* Money system; Units, Tens, Hundreds, Thousands, Millions, &c. of *Pounds*, requiring no change whatever, *nor should any whatever be attempted, as to them.*

"I shall feel most particularly obliged if you would hand or inclose these Papers, to such of His Majesty's Ministers as they most concern, and render the public as well as me, all the service you can, towards accomplishing this object, which I feel confident, will meet ready and general approbation.—I particularly hope, that the *English Names* of Pound, Tenth, Hundredth, and Thousandth may be adopted for the Coins, without *Latin* or other foreign prefixes to the word *Pound;* because, when new Measures and Weights are introduced, *they should have foreign prefixes* (and I hope those used in France) as they *must have* double or *compound Names*, to distinguish the four various classes; viz. Measures *of length*, *of surface*, *of solidity*, and *Weight;* yet for our Money, if English Names are used, they need not, *in common use at least*, be compound, or have Pound added, but Tenth, may always mean 2*s*. Hundredth, the $\frac{1}{10}$th of this, and Thousandth, the new substitute for Farthing.

"I hope it will not be too late, *in part at least* of the intended issue of new *Shillings* and *Sixpences*, to put on them the inscriptions, 5 *Hundredths*, and 25 *Thousandths*, respectively; as this would be *of very considerable use*, and could do no possible harm. "I am," &c.

"*Mr. Farey's Suggestions, as to the issue of Decimal Money.*

"Mr. Farey senior (of 37, Howland-street, Fitzroy-square) in a Report to the Board of Agriculture on *Derbyshire*, the 3rd volume of which is now just printed, and will, as he expects, be published *in a few days*, has entered into the subject of *Measures, Weights, and Money*, with somewhat different, and as he hopes considerably more *practicable views*, than many others have done.—In pp. 465 and 466 (printed off in March last) his observations as to *Money*, are as follows; viz."

'Lastly; as to Money, we fortunately now use in Accounts, only the four denominations, *Pounds*, *Shillings*, *Pence*, and *Farthings;* amongst these, *the Pound Sterling* is so much more generally and importantly used, than either of the other three denominations, that no hesitation can take place, in proposing this as *the unit of Money*, or circulating medium of value; and fortunately, *Two Shillings* is the exact $\frac{1}{10}$th of this unit, as follows; viz.

 '$\frac{1}{1000}$,

' $\frac{1}{1000}$, $\frac{1}{100}$, $\frac{1}{10}$ (Two Shillings), 1 (*Pound*, Sterling), 10, 100, 1000, 10,000, 100,000 (Plum), 1,000,000 (Million, Sterling), &c.

' One hundredth of a Pound ($\frac{1}{10}$th of two Shillings, or of 48 half-pence), is so near to 5 half-pence, and $\frac{1}{10}$th of these last being a Farthing, no serious inconvenience or injustice could follow, on the enacting of the above decimal divisions of the Pound, by appropriate names, and issuing Coin corresponding thereto, to permit, for one or two years, a Farthing to pass (*in change only*), for $\frac{1}{1000}$dth of a Pound, and 5 half-pence (or $2\frac{1}{2}$d of the present coin), instead of the $\frac{1}{100}$dth of a Pound, before the present Copper Money need wholly be called in; and so, without any harm, Sixpences, Shillings, Eighteenpences, Half-Crowns, and Three-Shilling Pieces, might, for a time at least, continue to circulate with the new Decimal Coin, as the $2\frac{1}{2}$–100dths, 5–100dths, $7\frac{1}{2}$–100dths, $1\frac{1}{4}$–10ths, and $1\frac{1}{2}$–10ths, respectively, of the Pound Sterling.

' This reformation of our Money, and enabling all Accounts to be kept *in one column*, instead of three, just as the Pounds are at present, and rendering unnecessary, ' Reduction of Money,' now so formidable a rule to Youths, in our Elementary Books of Arithmetic, and so troublesome in business, is indeed so easy to be accomplished, that it were extremely desirable Government would take it up, separately from, and previously to, reforming the Measures and Weights, on similar principles.'

" And in the concluding Chapter, p. 681, Note, now printing off (14th June), after some remarks in favour of proposing and adopting *decimal scales, and these only*, whenever Weights, Measures and Monies shall *be attempted to be altered*, from their present denominations, his remarks are as follows; viz."

' The call now (June 1816) so strongly expressed, for a Coinage of *Pound Pieces* of Gold, instead of Guineas (which once prevailed amongst us) comes most opportunely, for the adoption abovementioned; and I would beg earnestly to press on the attention of His Majesty's Ministers, the propriety and the great advantages which will result, from accompanying the issue of these new *Pound-pieces*, by moderate numbers of *Tenth-pieces*, *Hundredth-pieces*, and *Thousandth-pieces*, of a Pound, for circulation, *along with the present* (or new) *Silver and Copper Coins*, as recommended in p. 465, until the Public are fully habituated to their relations, and see by use, the utility of *a decimal scale of Money*, agreeing with that of our numeration and arithmetic, as ere long, I hope, it will also do, with all our Weights and Measures.'

" The present and new Coins would then stand related as follows: viz.

GOLD.

										Decimal Account.
GOLD.	*Guinea*	=	$1\frac{1}{20}$ Pound	=	$10\frac{1}{2}$ Tenths	=	105 Hundredths	=	1050 Thousandths	1·050
	(Twenty Shillings)	=	1 POUND	=	10 Tenths	=	100 Hundredths	=	1000 Thousandths	1·000
	Half-Guinea	=	$\frac{21}{40}$ Pound	=	$5\frac{1}{4}$ Tenths	=	$52\frac{1}{2}$ Hundredths	=	525 Thousandths	·525
	Seven Shillings	=	$\frac{7}{40}$ Pound	=	$3\frac{1}{2}$ Tenths	=	35 Hundredths	=	350 Thousandths	·350
SILVER.	*Crown*	=	$\frac{1}{4}$ Pound	=	$2\frac{1}{2}$ Tenths	=	25 Hundredths	=	250 Thousandths	·250
	Three Shillings	=	$\frac{3}{20}$ Pound	=	$1\frac{1}{2}$ Tenth	=	15 Hundredths	=	150 Thousandths	·150
	(Two Shillings)	=	$\frac{1}{10}$ Pound	=	1 TENTH	=	10 Hundredths	=	100 Thousandths	·100
	Eighteen-pence	=	$\frac{3}{40}$ Pound	=	$\frac{3}{4}$ Tenth	=	$7\frac{1}{2}$ Hundredths	=	75 Thousandths	·075
	Shilling	=	$\frac{1}{20}$ Pound	=	$\frac{1}{2}$ Tenth	=	5 Hundredths	=	50 Thousandths	·050
	Six-pence	=	$\frac{1}{40}$ Pound	=	$\frac{1}{4}$ Tenth	=	$2\frac{1}{2}$ Hundredths	=	25 Thousandths	·025
COPPER.	$(2\frac{1}{2}$d $- \frac{1}{10}$d)	=	$\frac{1}{100}$ Pound	=	$\frac{1}{10}$ Tenth	=	1 HUNDREDTH	=	10 Thousandths	·010
	Penny	=	$\frac{1}{240}$ Pound	=	$\frac{1}{24}$ Tenth	=	$\frac{5}{12}$ Hundredth	=	$4\frac{1}{6}$ Thousandths	·004,066$\dot{6}$
	Half-penny	=	$\frac{1}{480}$ Pound	=	$\frac{1}{48}$ Tenth	=	$\frac{5}{24}$ Hundredth	=	$2\frac{1}{12}$ Thousandths	·002,083$\dot{3}$
	Farthing	=	$\frac{1}{960}$ Pound	=	$\frac{1}{96}$ Tenth	=	$\frac{5}{48}$ Hundredth	=	$1\frac{1}{24}$ Thousandths	·001,041$\dot{6}$
	$(\frac{1}{4}$d $- \frac{1}{100}$d, $=$ 1f $- \frac{1}{25}$f)	=	$\frac{1}{1000}$ Pound	=	$\frac{1}{100}$ Tenth	=	$\frac{1}{10}$ Hundred	=	1 THOUSANDTH	·001

"Mr. Farey has little wish to go further at present, (or very little) than *offering this plan and the four new Coins*, to the Public, for their acceptance and adoption, reserving further proceedings thereon, until *a complete decimal scale of Measures, and Weights* also is matured, and in action; and therefore, it would, he suggests, be proper to enact,

"1st. That no persons should be entitled *to demand* any of the new decimal Coins, instead of the present legal ones, if such are tendered.

"2d. That a *Pound-piece*, might legally be tendered and given in payment, instead or in lieu of 20 shillings.

"3d. That a *Tenth-piece* might be tendered and given instead of Two Shillings, or of 4 Six-pences, or of 24 Pence, &c.

"4th. That a single *Hundred-piece* might be given in lieu of Twopence-halfpenny, or 10 Farthings; two of such for five-pence, or 20 Farthings; three of such and a one Thousandth-piece for sevenpence-halfpenny, or 6 pence and 3 halfpence; four of such and a one Thousandth-piece for Tenpence, or 6 pence and 4 pence; and beyond which number of four Hundredth-pieces, in any one payment, no person should be compellable to take them, instead of the present Coins.

"5th. That a single *Thousandth-piece* might be given in lieu of a Farthing, two of such for a half-penny, three of such for three Farthings, and so on, to ten of such Thousandth-pieces (which together are equivalent to Twopence-halfpenny, within *less than the half a farthing*), beyond which number of ten Thousandth-pieces, no person should be compellable to take them, instead of the present Coins.

"Mr. Farey begs further to remark, that his proposal, as above, has no dependance on the questions, as to how great a weight of Gold, of Silver, or of Copper, or how fine, should be put into the four new Coins, respectively; as he assumes, that the Pound will nominally and legally, be equal to 20 shillings, 240 pence, and 960 Farthings respectively; and the new Coin, exact decimations of each other.

"He cannot avoid expressing a hope, that no propositions will be listened to, for *destroying all our present Coins* and the Pound sterling with them, in favour of exact *aliquot parts* of a Troy pound of Gold, or of Silver, or the like; much less, the still more improper proposal of some, for destroying all these, except *the Farthing*, in order from this very trifling *unit*, rather than the Pound Sterling, to raise *a decimal scale of Monies*, which in the ordinary transactions of business, would from the greatness of the numbers, be intolerable, and how much more so in the Public accounts?"

"SIR,

"37, Howland-street, Fitzroy-square, June 16, 1816.

"Sir,—The objection which you so strongly stated in our last conversation, to the introduction of the decimals of the Pound sterling; viz. that Persons whose dealings were *wholly* effected in Copper Money, without the aid of Silver Coin (which is a very extreme case) would lose 10 pence in every Pound Sterling, or $4\frac{1}{6}$ per cent. by taking *Thousandths* of a Pound in lieu of *Farthings*,—has suggested to me a remedy, which I beg now to submit for your consideration: viz. That a new denomination, and small Coin should be temporarily introduced, which might be called *Mites*, each $\frac{1}{25}$th part of a Farthing in value, which would admit of calculations, and payments being made, and of accounts being kept in 3 columns *£. s. d.* as at present, or *exactly equivalent* calculations, payments, and accounts, in two new Columns, Pound-decimals and Mites, or *£. M.* as in the following Table; viz.

An exact Equivalent Table, *for keeping Accounts, either in Shillings Pence and Farthings, as at present, or in* Decimals of a Pound, *and* Mites, *each $\frac{1}{25}$th of a Farthing.*

Old Mode.		New Mode.		Old Mode.		New Mode.		Old Mode.		New Mode.		Old Mode.		New Mode.	
Shillings.	Pence & Farthings.	Pound Decimal.	Mites.	Shillings.	Pence.	Pound Decimal.	Mites.	Shillings.	Pence.	Pound Decimal.	Mites.	Shillings.	Pence.	Pound Decimal.	Mites.
S.	*D.*	*£.* te.hu.th	*M*	*S.*	*D.*	*£.* te.hu.th	*M*	*S.*	*D.*	*£.* te.hu.th	*M*	*S.*	*D.*	*£.* te.hu.th	*M*
0	0¼	·001	1	0	6¼	·026	1	1	0¼	·051	1	1	6¼	·076	1
0	0½	·002	2	0	6½	·027	2	1	0½	·052	2	1	6½	·077	2
0	0¾	·003	3	0	6¾	·028	3	1	0¾	·053	3	1	6¾	·078	3
0	1	·004	4	0	7	·029	4	1	1	·054	4	1	7	·079	4
0	1¼	·005	5	0	7¼	·030	5	1	1¼	·055	5	1	7¼	·080	5
0	1½	·006	6	0	7½	·031	6	1	1½	·056	6	1	7½	·081	6
0	1¾	·007	7	0	7¾	·032	7	1	1¾	·057	7	1	7¾	·082	7
0	2	·008	8	0	8	·033	8	1	2	·058	8	1	8	·083	8
0	2¼	·009	9	0	8¼	·034	9	1	2¼	·059	9	1	8¼	·084	9
0	2½	·010	10	0	8½	·035	10	1	2½	·060	10	1	8½	·085	10
0	2¾	·011	11	0	8¾	·036	11	1	2¾	·061	11	1	8¾	·086	11
0	3	·012	12	0	9	·037	12	1	3	·062	12	1	9	·087	12
0	3¼	·013	13	0	9¼	·038	13	1	3¼	·063	13	1	9¼	·088	13
0	3½	·014	14	0	9½	·039	14	1	3½	·064	14	1	9½	·089	14
0	3¾	·015	15	0	9¾	·040	15	1	3¾	·065	15	1	9¾	·090	15
0	4	·016	16	0	10	·041	16	1	4	·066	16	1	10	·091	16
0	4¼	·017	17	0	10¼	·042	17	1	4¼	·067	17	1	10¼	·092	17
0	4½	·018	18	0	10½	·043	18	1	4½	·068	18	1	10½	·093	18
0	4¾	·019	19	0	10¾	·044	19	1	4¾	·069	19	1	10¾	·094	19
0	5	·020	20	0	11	·045	20	1	5	·070	20	1	11	·095	20
0	5¼	·021	21	0	11¼	·046	21	1	5¼	·071	21	1	11¼	·096	21
0	5½	·022	22	0	11½	·047	22	1	5½	·072	22	1	11½	·097	22
0	5¾	·023	23	0	14¾	·048	23	1	5¾	·073	23	1	11¾	·098	23
0	6	·024 ·025	24 0	1	0	·049 ·050	24 0	1	6	·074 ·075	24 0	2	0	·099 ·100	24 0

 "Of

"Of which Table, and of 9 other similar and succeeding pages, a great number of stereotyped copies should be issued.

"The *large* figures show the *Coins* that are now, and should I suggest be put speedily into circulation: the five first of the new Decimal Monies, should be of Copper, and be inscribed on one side respectively, *One Thousandth*, *Two Thousandths*, *Four Thousandths*, *One Hundredth*, and *Two Hundredths*; and two others of Silver, inscribed *Four Hundredths* and *One Tenth*, respectively*.

"The 5 new small Coins (as only for *temporary* use) might be of Iron and very thin, so as almost to equal a farthing in diameter, and have inscribed on one side *One Mite*, *Two Mites*, *Four Mites*, *Ten Mites*, and *Twenty Mites*, respectively; and around these latter inscriptions, it would be proper to circumscribe, *with one Thousandth, makes a Farthing*; *with Two Thousandths, makes a Halfpenny*; *with Four Thousandths, makes a Penny*; *with One Tenth, makes Twopence-halfpenny*; and *with Two Tenths, makes Fivepence*, respectively; by which means, the uses and values of these temporary Coins, would be soon understood by every one, and the above Coins would suffice, for readily making up any exact sum required.

"As fast as bargains and dealings came to be made in the Decimal Money, the Mites (and their column, in accounts) would be laid aside; and after a sufficient time, when this came to be almost wholly the case, in consequence of further issues of the new Coin, all the present Coin might be called in, and the use of three troublesome columns in accounts, wholly done away; and what is of even greater consequence, every *price* will be ready *without any reduction*, for multiplying by its *quantity* of a commodity (these also to be decimal) and the result would in no case need any reduction, advantages that would be incalculable, in a trading and scientific community.

"The trouble of such small Coin, and of a Column, of very trifling amount in most cases, to carry at 25, would, I think, operate favourably, to induce bargains to be made and prices of articles to adjust themselves, ere long, to the Decimal Money; Trustees of Turnpike Roads and Bridges having their Tolls *fixed* and mostly paid in Copper, (who seem to have the greatest difficulties to surmount) might be induced to lower the Tolls under 6d (and odd copper, above it) to the decimal Money, on being allowed by the Quarter Sessions, on production of correct accounts of their separate Tolls, to advance the higher Tolls on Carriages,

* "Some may perhaps think it necessary, to issue three other new Coins; viz. for *Twenty-five Thousandths*, *Five Hundredths*, and *Seventy-five Thousandths* of a Pound, respectively.

as

as much as to counterbalance the loss on the small ones, (on Horses and foot-passengers, &c.) by their being made Decimal.

"Canal Tolls, as well as the Customs and other Taxes to Government are many of them charged in small money, not exactly reducible to *decimal* Money, yet these tolls and duties are always *paid*, on pretty *considerable quantities;* so that the giving up of the *fraction of a farthing in the whole amount*, on either side, so as to Receive in Decimal Money, could not be an object of material consequence; indeed, whenever the Mites should exceed 12, one more Thousandth might be payable, in this and other cases, which, in the long-run, would balance the errors. I shall be happy to give any further explanations in my power, before leaving Town, which I hope will be in the end of the week, Mr. McMillan now having the Index to my last volume of Report, in his press.

"I am, &c."

III. *On the Article in our last Number, entitled "Controversy concerning Safe-lamps."* *By* W. P. Knight, *Esq.*

To Mr. Tilloch.

Sir,—I perused, with a certain degree of surprise, an article in your excellent Magazine, entitled "Controversy concerning Safe-lamps," which you introduce by expressing an opinion that "you had conceived that the discoveries of Sir H. Davy and Mr. George Stevenson were made independent of each other." I do not think the question of dates so near to each other of any importance; and I am still inclined to think that Mr. Stevenson began, without knowing what Sir H. Davy was doing, though he probably did hear that he was engaged in the inquiry.

When I first saw the account of Mr. G. Stevenson's lamp in the Philosophical Magazine, I had an idea that it was a lamp which might burn safely in explosive atmospheres: but having seen the lamp during a journey that I made in the north of England this summer, I was obliged to give up this opinion; and since I have read Mr. R. W. Brandling's letter, I am convinced that Mr. Stevenson never made any discovery, and that there is not the slightest analogy between what he attempted and that which Sir H. Davy discovered and carried into execution.

It is said "that Mr. G. Stevenson had conceived an idea that hydrogen gas might be admitted into a lamp so as to be gradually consumed." If any thing be meant by this idea, it must be—that explosive mixtures admitted into a lantern in small

quantities at a time, will not explode, but be gradually consumed by combustion—which is utterly false. Whatever quantity of explosive gas is admitted to the contact of flame, it will explode, that is, it will burn at once, and not be gradually consumed; and in the wire-gauze safe-lamp the whole cylinder is filled with flame.

But on this erroneous idea Mr. G. Stevenson, it is said, endeavoured to construct a safe-lamp: First, by admitting air by one tube, then by three tubes (which Mr. R. Brandling calls capillary, though no dimensions are stated), and last of all, by a row of small holes on the outside of a copper lamp covered with a cylinder of glass. This last is the only lamp which has been shown in public. It is on this, therefore, that I shall make my observations. The holes in that which I saw were at least the 1-12th of an inch in diameter below, and more than the 1-6th of an inch in diameter above, and therefore would readily communicate explosion: the glass cylinder was loose, and there was nothing to prevent the communication of explosion through it. I never heard that this lamp was exposed to any other proof than that of sending pure fire-damp into it, which would extinguish the candle in a common lantern; or of being placed in the upper part of a board, where the pure fire-damp immediately extinguished it. It is certain that if an explosive mixture ever began to burn in this apparatus, it must communicate explosion to the outward atmosphere. I shall now say something of Sir H. Davy's principles, which were made known to scientific men, and pretty generally communicated, whilst Mr. G. Stevenson's were in embryo, or confined to the Killingworth Colliery.

Sir H. Davy's first idea of a safe-lamp was founded on a discovery which he made—that a very small admixture of azote prevented the explosion of detonating mixtures of fire-damp*. Hence he made a light burn in a close lamp supplied with a limited quantity of air; and when this air was further deteriorated by an admixture of fire-damp the light was extinguished. This lamp, though the principle of it was strictly philosophical, could not have been very useful, for it must have been extinguished by a very slight admixture of fire-damp in air.

I must, however, vindicate this first principle of Sir H. Davy against an enlightened and liberal advocate of his cause, the Rev. John Hodgson, who says that Mr. John Murray thought of a diminished atmosphere: whereas, as far as I can learn from the perusal of his modest and luminous little treatise, he only anticipated his namesake (Dr. Murray) and Mr. R. W. Brandling in the idea of a tube for bringing the air from a distance.

* See Phil. Mag. for Dec. 1815.

The

The great principle of security that Sir H. Davy discovered, is, that certain cooling surfaces used for transmitting gas destroy the power of communicating explosion. To this no other person can lay claim: and it required great sagacity, and multiplied and varied experiments, to trace this principle from its operation in long metallic tubes and canals, to that in a tissue permeable to light and air, and impermeable to flame.

Capillary tubes or narrow canals, as Sir H. Davy has shown, are not safe as such. He has demonstrated that their power of preventing the passage of explosion depends upon their number: nor are numerous small apertures safe, unless they exist in a whole surface. The explosion of hydrogen, it is well known, will pass through an aperture of 1-70th of an inch in diameter, or through a glass tube 1-40th of an inch diameter and six inches long.

Mr. R. W. Brandling is of opinion that wire-gauze is only the extremities of capillary tubes:—this opinion is too ridiculous to merit serious notice. It is singular that Mr. R. W. Brandling has not discovered the principle of the wire-gauze safe-lamp in the common wire firescreen. According to such analogical reasoning, the steam-engine would be only the prolongation of a teakettle, and the air-pump an improvement of the wet leather used by children for raising smooth stones.

I know from very good authority that Mr. R. W. Brandling was present when a vote of thanks was unanimously passed to Sir H. Davy by the coal trade, for his brilliant discovery of the wire-gauze safe-lamp: nor did he set up any claims for Mr. Stevenson till some months after, when a present of plate was in contemplation:—and it should be published for the honour of the coal owners, that on this occasion his was the only disputant voice.

Mr. R. W. Brandling is himself the constructor of a *rival* lamp, and on such an occasion he cannot be regarded as a very unprejudiced advocate, or even witness; and, except to those who are very ignorant of the most common principles of chemistry, there can be no question of analogy between the attempts of Mr. Stevenson, as they have been justly called by the committee of the coal trade (however laudable), and the scientific discoveries of Sir H. Davy.

Mr. G. Stevenson may (as Mr. Hodgson says in his candid and gentleman-like communications) be a very modest and ingenious man. As an engine-wright I dare say he is superior to most who exercise that occupation; and Mr. R. W. Brandling can only do harm to a useful and industrious mechanic, by persuading him to set up as a philosophical discoverer.

It cannot seem extraordinary to any one who has considered the

the late progress of chemical philosophy, that the discoverer of the laws of electrical decomposition; of the composition of the alkalies, earths and acids, supposed to be simple; of the independence of the supporters of combustion; should have developed a new chemical principle with respect to flame: or that he should have accomplished an object on which he was employed with great zeal and unwearied industry for some months.

Though I have taken great pleasure in chemical inquiries, I am not known as a publisher; nor should I have volunteered any observations on this occasion, had I not been deeply penetrated by the greatness of the benefits conferred on humanity by the invention of the wire-gauze safe-lamp, during an examination that I made of some of the mines in the neighbourhood of Newcastle. I saw some hundreds of the lamps in the hands of the miners, who spoke of them with a mixture of gratitude and affection under the name of *Davies*. I should feel unworthy of perusing works of philosophy, if I did not find my heart glow at such a sight, and if I did not feel some indignation when I hear the merits of such a benefactor to society depreciated.

Our great English chemical philosopher by his various brilliant discoveries laid long ago the foundation for an unperishable fame; and if his discovery of the chemical agencies of electricity be regarded as the solid base of the column of his scientific glory, the discoveries which terminated in the safe-lamp may be regarded as its Corinthian capital, equally beautiful and useful; and this column will stand "untouched by time," when the rubbish and dirt which envy and jealousy heap around it shall decay and disappear:—and even should this generation neglect it, posterity will not fail to crown it with *civic* garlands, in which the evergreen oak and laurel are blended, emblems of a triumph of genius and labour over one of the most destructive agents with which humanity had ever to contend.

I am, sir,

Your obedient humble servant,

Chelsea, Dec. 16, 1816. WM. POWELL KNIGHT.

IV. *On Vision: in Answer to Mr.* PATER. *By Mr.* ANDREW HORN.

To Mr. Tilloch.

SIR, — WHETHER the ignorance and inconsistency manifested by Mr. Pater, in your number for November, be real or assumed, is of little importance. One thing is certain, that "good humour" never can atone for these blemishes in any writer. I hazard the assertion, that there is not one among the numerous readers of the

the Philosophical Magazine, except Mr. P., that would confound "*my idea*" of the manner in which vision is accomplished, with that of "*his friend*," who seems to have understood nearly as much of *optics* as himself. Much as it may surprise Mr. P., I can assure him, that *my* theory is verily built upon *his* "old opinion, that *light* acts upon the *optic nerve*, and *excites sensation*"—*there*, and not upon the *retina*.

In my former communication (No. 220, p. 117), I merely stated the principle of my theory, and introduced an experiment which I had made in support of that principle. Should any gentleman *scientifically* answer my arguments, and invalidate the experiments adduced in the pamphlet, where the theory is fully developed, I should at once renounce it as an untenable hypothesis; but till then I am warranted in retaining an opinion sanctioned by high philosophical authority, that "the phænomena are more philosophically explained by this theory than any hitherto proposed."

In the whole circle of science, no theory has obtained more admiration than that of light and colours, as established by Newton upon his experiments with the prism. But it certainly owes more to the mathematical genius of its author than to the truth of its physical principles. He assumes it as a first principle, supported by mathematical calculation, that the prismatic *spectrum* is an *image* of the sun; and then proceeds, with the same rigid adherence to mathematical demonstration, to show, that the prism *decomposes* the rays that enter the foramen in the window-shutter, so as to produce a *spectrum* whose length is about five times its breadth, and composed of seven colours. This altogether is taken as a demonstration that light is composed of so many different kinds of rays, that differ in refrangibility in the same order in which they stand to the refringent angle of the prism. In the formation of the spectrum we are to suppose an infinite number of circles, composed of rays differently refrangible, and projected a little beyond each other, so that the assemblage of circles forms an oblong image, the extremities only of which are circular.

But the principle is fallacious, that supposes the prismatic spectrum to be an *image* of the *sun*, when it is evidently an image of the *hole* in the window-shutter, with its polarized light. As for the lengthening of the spectrum, there is no proof that the rays are decomposed and separated by the prism. The fact is, that the prism modifies the direction of the rays coming from the aperture, which otherwise would be *dispersed;* so that, so far is the prism from *decomposing* the rays, it *collects* and transmits them in such directions as to mix and compound them in the spectrum. Having made these remarks, I shall now endeavour to demonstrate how the colours in the prismatic spectrum are produced.

produced. Let *a c* (Plate I. fig. 9.) represent an aperture in the window-shutter of a darkened room, through which *n o*, a beam of solar light, is transmitted; and *d e f*, a prism placed behind the aperture *a c*. Now the ray *a c* is not attracted, but repelled at the lower edge of the hole, towards the higher angle of the prism *d*; in like manner, the ray *n a* is refracted from the upper edge of the aperture towards *e*, the refringent angle of the prism, and becomes incident at r''. It is evident that these two rays are most refracted, and have the greatest *polarity* of all the rays that compose the beam of light; and therefore, as the angle of refraction is in each nearly equal, both rays will be *red*. The intermediate rays, incident upon *d e*, will give a succession of colour according to their refraction or polarity; the ray next to r' will be *blue* b', that next to b' will be *yellow* y'. The same order will follow with relation to the *red* ray at the refringent angle; the ray b'', that falls next to r'', is *blue*; and that next to b'' is y'', or *yellow*. All these rays, r' b' y', y'' b'' r'', pass through the planes of the prism with a refraction corresponding to the angles of their incidence; and, agreeably to a well-known law of optics, the angle of their emergence, at the opposite surface, *c e*, of the prism, is respectively as the angle of their incidence. Hence, on examining, the spectrum r' is incident at V, b' at B, and y' at Y; the ray r'' falling with somewhat more obliquity upon the plane *d e* than the ray r', a portion of it is reflected in the direction *x*,—while the other passes through the opposite plane, and is incident at R. Now it is evident that the *red* which the spectrum here exhibits, is produced by the ray $a\,r''$ that comes from the upper edge of the aperture; the *orange*, which appears next the red, at O, is a composition of the rays r'' and y'; the *yellow*, which is exhibited at Y, is produced by the mixture of the rays y' and y''; the *green*, at G, is composed of the rays b' and y''; the *blue*, which is exhibited at B, is produced by the ray b'; the *indigo*, at I, is a mixture of the rays r', b'', b''; and the *violet*, at V, is produced by the rays r' and b''. There is also in the centre of the solar beam, when the aperture in the window is not too small, a colourless or direct ray, which cannot enter the spectrum, it is always lost in taking the straight direction, *m*.

This theory of the prismatic spectrum also scientifically explains why it is an inverted image; and, on the legitimate principles of optics, accounts for the diversity of the spaces which the different colours occupy in it, which cannot be done upon the received hypothesis. Newton supposes the rays to cross *before*, instead of *after*, they enter the aperture through which they are transmitted. The heating power, and various chemical effects produced by the prismatic light, may be resolved into three causes:—the difference of polarity in the rays, the difference

difference of their velocity in the planes which they traverse, and the diversity in the mixture of the rays forming the spectrum.

I am, sir,

Your very obedient servant,

Wycombe, Dec. 10. 1816. ANDREW HORN.

V. *On Mr.* GEORGE STEVENSON'S *Pretensions to the Discovery of the Safe-lamp. By* A CORRESPONDENT.

To Mr. Tilloch.

SIR, — I PRESUME, when you say in the last number of the Philosophical Magazine that you had conceived that Mr. George Stevenson's and Sir H. Davy's discoveries were made independent of each other, that you must allude to Sir H. Davy's first lamp, with apertures in the bottom and top; for I cannot suppose that you can mean to establish an analogy between Sir H. Davy's wire-gauze safe-lamp, and any thing that Mr. G. Stevenson has attempted.

The only date of Mr. G. Stevenson's that can be considered as of any value, is that of Nov. 24, which is more than a fortnight after Sir H. Davy's first paper, and a week after his second paper had been read to the Royal Society, and twenty days after Sir H. Davy's discovery had been made known to the coal-trade, and a month after it had been stated publicly to the Chemical Society of London, "that if a lamp was supplied with air through small apertures only, below, explosive atmosphere would extinguish the flame," and "that explosive atmospheres could not transmit the explosion through small tubes."

Mr. G. Stevenson says, that he only profited in the practice of his lamp by the publications of scientific men; and it is pretty evident, from the circumstances of dates, that he profited by what Sir H. Davy had published, though certainly the profit was very small.

Mr. G. Stevenson is said to have embraced the idea of admitting hydrogen in small quantities, so as to consume it by combustion, and this in a close vessel. Nothing can be more absurd than the idea. He attempted this with a tube and a slider as late as Nov. 17, long after Sir H. Davy had shown and published that capillary tubes, of a certain diameter and in certain numbers, were safe, and that a lamp supplied with air through small apertures in the bottom was safe.

Mr. G. Stevenson's friends at first were *very anxious* to show that his lamp was not stolen from Sir H. Davy's. When, on Nov. 3, Dr. Grey and the Rev. J. Hodgson spoke of Sir H. Davy's discoveries,

discoveries, Mr. Lambert of Killingworth Colliery said that Mr. Stevenson had made a safe-lamp : but it is evident from his own confession, that this was the lamp with a single tube and a slider, upon a principle which had been recommended by John Murray, esq.

But, sir, what can be more vague than the certificate? *Trials* are said to have been made, but not how. Hydrogen is talked of, explosive mixtures of which will pass through tubes much too fine to supply air to a light, unless there were scores of them.

Mr. W. Brandling himself, the inventor of a safe-lamp, is the advocate for Mr. G. Stevenson. If Mr. W. Brandling knew, Nov. 24th, that Mr. G. Stevenson had discovered a safe-lamp, why did he himself, in December, bring forward a lamp with a bellows and a tube at the bottom? And why did he not oppose the vote of thanks at which he was present, March 4th, when it was unanimously agreed that the united thanks and approbation of all present should be given, for the great and important discovery of a safety-lamp for exploring mines charged with inflammable gas, and when they expressed their admiration of the talents and acquirements which had achieved so momentous and important a discovery?

These, sir, are convincing facts. It is evident that even now neither Mr. W. Brandling nor Mr. G. Stevenson has any scientific ideas on the subject of safe-lamps. If they were so well informed on the subject of flame, why did they not invent the *wire-gauze* safe-lamp, which Sir H. Davy himself did not discover till two months after his researches were begun, and which was attained by the natural course of analogy, guided and confirmed by experiment and improved observation?

Because an ingenious and illiterate mechanic conceives that a light will burn from hydrogen without atmospherical air, and tries by a tube with a slider to verify this happy idea—therefore the inventor of a lamp composed of a tissue permeable to light and air, and impermeable to flame, (by which the fire-damp itself is consumed and made to give light, and which is daily giving security to hundreds of miners,) is entitled to no praise from the coal owners, but is to be calumniated by a man who is constantly profiting by his labours!!

If the wire-gauze safe-lamp had failed, who would have borne the blame? Mr. G. Stevenson or Mr. W. Brandling? or any of those persons so anxious at first to prove it of no value, and then so desirous of proving that it was no discovery? No; the real inventor and the first victims would have been those enlightened gentlemen who trusted their lives to the resources of science, and made the first experiments in mines with it; the Rev. John Hodgson, Mr. Buddle, Mr. Murray, Mr. Peele. Now that it is in

in the hands of hundreds, it has ceased to be a wonder to Mr. G. Stevenson, who, it is well known, when he first saw it, said that it could not possibly succeed.

I am, sir,

Your obedient humble servant,

London, Dec. 20, 1816. PHILALETHES.

VI. *Remarks on Mr.* LAPLACE'S *Table of the Depression of Mercury in the Tubes of Barometers**.

To Mr. Tilloch.

SIR,—MR. LAPLACE speaks in this paper of having "discovered" and "reduced to the fundamental principle of chemical affinities" the theory of capillary action: at least there can be no doubt that his expressions are meant to be understood as relating entirely to his own investigations. I must confess that I have never thought his reasoning on the corpuscular powers of matter sufficiently demonstrative: but it is only at this moment that I have been able fully to satisfy myself, by a different train of arguments, with respect to the true foundation of the laws of capillary action.

It has been shown by an anonymous writer in Nicholson's Journal for September 1807, that the superficial strata of cohesive bodies, of a thickness less than the distance to which the cohesive force extends, must be less dense, and less coherent with the strata beneath them, than other parts of the body. Hence it may be inferred that a small portion of a liquid, in the middle of such a stratum, will be less strongly pressed above and below than a similar portion of the internal part of the liquid, while the lateral pressure is undiminished, the particles in the direction of the surface on each side exerting their full cohesive power. This inequality must necessarily occasion a tendency in the stratum to acquire a greater thickness and a less length, or in other words a contractile force, which will be the same in every part of the surface, and which will sufficiently explain all the phenomena of capillary action.

Mr. Laplace has indeed denied the existence of such a tension: but there is some reason to apprehend that he may have been since disposed to retract this opinion; as he most assuredly would disclaim the preference which he has given to the slovenly method in which his table is computed, if he should ever have an opportunity of comparing the accuracy of its results with the details of the same calculation by means of the series published

* See Phil. Mag. for February 1816.

in this country, especially if a few more of its coefficients were calculated; the convergency of the quantities concerned affording an infallible test of the greatest possible amount of error.

I am, sir,

Your very obedient servant,

London, 4 Jan. 1817. E. F. G. H.

VII. *Some Account of the White Mountains of New Hampshire. By* JACOB BIGELOW, *M.D. Lecturer on Materia Medica and Botany, and Rumford Professor in Harvard University* *.

THE terms mountain and hill are words altogether relative in their signification, and are variously used in different parts of the world, according to the experience and conceptions of those who apply them. In this country, elevations which are known only as hills, would in Great Britain assume the character of mountains; while on the other hand, our highest summits dwindle to an inferior size, when contrasted with the peaks and ridges of Switzerland, of Tibet, and Peru. The face of the country in many parts of the United States is uneven, rugged, and precipitous; its chains of highlands occasionally shoot up into eminences, which are conspicuous at a great distance, and which are long and difficult of ascent. But the highest of these elevations has no claim to be considered a mountain even of secondary size, when compared with others which may be found in every quarter of the globe. The surface of the lake of Lausanice, in France, is higher than any mountain in the United States; and the city of Riobamba in Peru is built at an elevation more than twice as great †.

It is not, however, for their great elevation alone, that mountains become interesting to the naturalist and traveller. Those of minor or secondary size are equally objects of curiosity, and often furnish to the explorer more satisfactory results. The mediocrity of their height renders them of course more accessible, and more susceptible of investigation in all their parts. Being short of the limits of perpetual snow, they are covered with vegetables, wherever the earth on them is sufficiently deep. The prospect from such mountains, as Baron Humboldt has observed, is far more interesting than that from extreme elevations, where the scenery of the adjacent country is lost and confounded by the remoteness of its situation.

* From the New-England Journal of Medicine and Surgery for October 1816.

† Jameson's Mineralogy, vol. iii.

In

In the United States, exclusive, or possibly inclusive, of Louisiana, the highest point or ridge of land is undoubtedly that of the White mountains in New Hampshire. From the earliest settlement of the country these mountains have attracted the notice of the inhabitants, and of mariners along the coast, by the distance at which they are visible, and the whiteness of their appearance during three quarters of the year. They were for a long time the subject of fabulous representations; the Indians had a superstitious dread of them; and travellers who occasionally ascended their summits, returned with exaggerated reports of the difficulty and distance, as well as of the strange productions found on the more elevated parts of their surface.

The earliest account of an ascent of the White mountains is given in Gov. Winthrop's Journal, and appears to have taken place in the year 1642. This account is somewhat curious, if not otherwise, at least for its antiquity *.

* "One Darby Field, an Irishman, living about Piscat, being accompanied with two Indians, went to the top of the White Hill. He made his journey in eighteen days. His relation at his return was, that it was about 160 miles from Saco: that after forty miles travel, he did for the most part ascend; and within twelve miles of the top was neither tree nor grass, but low savins, which they went upon the top of sometimes, but a continual ascent upon rocks, on a ridge between two valleys filled with snow, out of which came two branches of the Saco river, which met at the foot of the hill, where was an Indian town of some 200 people. Some of them accompanied him within eight miles of the top, but durst go no further, telling him that no Indian ever dared to go higher, and that he would die if he went. So they staid there till his return; and *his* two Indians took courage by his example, and went with him. They went divers times through the thick clouds for a good space, and within four miles of the top they had no clouds, but very cold. By the way among the rocks there were two ponds; one a blackish water, and the other reddish. The top of all was plain, about sixty feet square. On the north side was such a precipice as they could scarcely discern the bottom. They had neither cloud nor wind on the top, and moderate heat. All the country about him seemed a level, except here and there a hill rising above the rest and far beneath them. He saw to the north a great water which he judged to be 100 miles broad, but could see no land beyond it. The sea by Saco seemed as if it had been within twenty miles. He saw also a sea to the eastward, which he judged to be the Gulf of Canada: he saw some great waters in parts to the westward, which he judged to be the great lake Canada river comes out of. He found there much Muscovy glass: they could rive out pieces forty feet long, and seven or eight broad. When he came back to the Indians, he found them drying themselves by the fire, for they had had a great tempest of wind and rain. About a month after, he went again with five or six of his company; then they had some wind on the top, and some clouds above them which hid the sun. They brought some stones which they supposed had been diamonds, but they were most crystal."—*Winthrop's Journal*, p. 247.

The relation of Darby Field may be considered as in the main correct, after making reasonable deductions for the distance, the length of the Muscovy glass, and the quantity of water in view, which it may be suspected has not been seen by any visitor since his time.

Within the last forty years the White mountains have been repeatedly ascended by different exploring parties, and several accounts of their productions and phænomena have been published. The object of this paper is to detail such observations as were made by a party from Boston, who visited them in the beginning of July of the last summer.

These mountains are situated in lat. about 44° 15′ N. and long. 71° 20′ W. from Greenwich. They are distant about 150 miles from Boston. Their Indian name, according to Dr. Belknap, was Agiocochook.

Our approach to them was made from the north-west, commencing at the town of Lancaster, a village situated on the Connecticut river, 25 miles from their base. From this town a road has been cut, passing through a gap of the mountains to Portland, and constituting the principal outlet of the Coos country. This road takes the course of the Israel's river, a branch of the Connecticut, passing between the Pliny mountains on the left, and the Pondicherry mountain on the right. The village of Lancaster is situated in a valley, surrounded in several directions by very elevated ridges of land. A number of the summits in sight of this place could not be estimated at less than 3,000 feet in height, judging from the experience we had acquired of several hills of known altitude on the road, and the accounts given by the inhabitants of the time necessary for their ascent and descent.

The road from Lancaster passes through Jefferson (formerly Dartmouth), Bretton woods and Nash and Sawyer's location, to the Notch of the mountains. This road in its course runs over the foot of the Pondicherry mountain. It lies for most of the way through thick woods, but rarely enlivened with the appearance of cultivation. At Playstead's house, thirteen miles from their base, we had a fair view of the White Hills. They presented the appearance of a continued waving range of summits, of which it was difficult to select the highest. At Rosebrooks, 4½ miles from the Notch, the view of them was very distinct and satisfactory. We could now clearly discern the character of the summits, five or six of which were entirely bald, and presented the appearance of a gray and ragged mass of stones towering above the woods, with which the sides and base were clothed. In several places we observed a broad continued stripe descending the mountain, and having the appearance of a regular road cut through the trees and rocks, from near the base to the summit of the mountain. On examining these with a telescope, they were found to be channels of streams; and in several, the water could be seen dashing down the rocks.

Between Rosebrooks and the Notch is a plain, or rather a swamp, the waters of which pass off in different directions, partly to

to the Ammunoosuck, a branch of the Connecticut, and partly by an opposite course to the Saco. After crossing several brooks running towards the former, we came to another stream, the water of which was so sluggish that it required some time to become satisfied that it was actually flowing in the opposite direction. This stream has its origin in a pond of one or two acres, situated near the road, and having no other inlet or outlet. This pond appears to be the principal source of the Saco river.

The waters of this stream being collected from several sources proceed directly toward the side of the mountain. At the point where to all appearance they must be intercepted in their course, there occurs one of the most extraordinary features of the place, well known by the name of the Notch. The whole mountain, which otherwise forms a continued range, is here cloven down quite to its base, affording a free opening to the waters of the Saco, which pass off with a gradual descent toward the sea. This gap is so narrow that space has with difficulty been obtained for the road, which follows the course of the Saco through the Notch eastward. In one place the river disappears, being lost in the caves and crevices of the rocks, and under the shelves of the adjoining precipice, at length reappearing at the distance of some rods below. The Notch gradually widens into a long narrow valley, in the lower part of which is situated the town of Bartlett.

There is no part of the mountain more calculated to excite interest and wonder than the scenery of this natural gap. The crags and precipices on both sides rise at an angle of great steepness, forming a support or basement for the lofty and irregular ridges above. One of the most picturesque objects in our view was a cliff presenting a perpendicular face of great height, and crowned at its inaccessible summit with a profusion of flowering shrubs*. For many miles below the commencement of the Notch the eye meets on both sides a succession of steep and precipitous mountains, rising to the height of some thousands of feet, and utterly inaccessible from the valley below. The sides of these mountains consist in some parts of bald rock, streaked or variegated by the trickling of water, in others they are covered with trees and shrubs. The occasional torrents formed by the freshets in the spring have in many places swept away the stones and trees from their course, for a great distance, and left the vestiges of their way in a wide path or gully over naked rocks.

* *Rhodora Canadensis,* in full flower June 20th.

In some instances the fire had run over the sides of the mountain, destroying the vegetation and leaving the dead trunks of the trees standing like stubble in a field, and presenting a singular appearance of desolation for some miles in extent. Several brooks, the tributaries of the Saco, fall down the abrupt declivities, forming a succession of beautiful cascades in sight of the road. We were told that the wind sweeps through the Notch at times with great violence. The lightning is said to strike frequently in the mountains from the clouds about their sides, and the sound of the thunder in this place is represented as unusually loud and severe. The report of a musket discharged in the Notch was followed by a long echo, reverberated for some time from both sides of the mountain.

The White Hills have been ascended by various routes, from their different sides. The course which is usually considered as attended with the least difficulties, is that which commences at the plain of Pigwacket, at present the town of Conway, and follows the course of Ellis river, a northern branch of the Saco, having its origin high in the mountain.

The place of leaving the road, to follow the track of this stream, is in the town of Adams, about twenty miles from the summit of the highest part of the mountain. Of this distance seven or eight miles may be rode over on horseback, the rest must be performed on foot. After leaving the borders of cultivation, our course lay through thick woods, on a level or with a gentle ascent, not much encumbered with an under growth of bushes, for six miles. The walking was tolerably good, except the circumstance of being obliged once or twice to ford the streams. Our encampment for the night was made at the mouth of New river, a principal branch of the Ellis. This river takes its name from the recency of its origin, which happened in October 1775. At this time, during a great flood that took place in consequence of heavy rains, a large body of waters, which had formerly descended by other channels, found their way over the eastern brink of the mountains, and fell down toward the Ellis, carrying the rocks and trees before them in their course, and inundating the adjacent country. By this freshet the banks of the Saco were overflowed, cattle were drowned, and fields of corn were swept away and destroyed. Since that period, the New river has remained a constant stream, and at the place where it descends the last precipice, forms a splendid cascade of 100 feet in height.

From this encampment, which was seven miles from the top of the mountain, we proceeded the next day (July 2) two or three miles by the side of Ellis river, on a gradual ascent, occasionally encumbered by the trunks of fallen trees. We now left the

the Ellis for one of its principal branches called Cutler's river, leading directly towards the principal summit. After climbing by the side of this stream for a considerable distance, the trees of the forest around us began to diminish in height, and we found ourselves at the second zone or region of the mountain. This region is entirely covered with a thick low growth of evergreens, principally the black spruce and silver fir, which rise to about the height of a man's head, and put out numerous, strong, horizontal branches, which are closely interwoven with each other, and surround the mountain with a formidable hedge, a quarter of a mile in thickness. This zone of evergreens has always constituted one of the most serious difficulties in the ascent of the White Hills. The passage through them is now much facilitated by a path cut by the direction of Colonel Gibbs, who ascended the mountain some years since.

On emerging from this thicket, the barometer stood at 25,93, giving our elevation above the sea at 4,443 feet. We were now above all woods, and at the foot of what is called the bald part of the mountain. It rose before us with a steepness surpassing that of any ground we had passed, and presented to view a huge, dreary, irregular pile of dark naked rocks.

We crossed a plain or gentle slope, of a quarter of a mile, and began to climb upon the side. There was here a continued and laborious ascent of half a mile, which must be performed by cautiously stepping from one rock to another, as they present themselves like irregular stairs, winding on the broken surface of the mountain. In the interstices of these rocks were occasional patches of dwarfish fir and spruce, and beautiful tufts of small alpine shrubs, then in full flower.

Having surmounted this height, we found ourselves on a second plain. This, like the first, was covered with withered grass and a few tufts of flowers. Its continuity is interrupted by several declivities, one of which we descended to our left, to reach a brook that crosses it here from the rocks above. There remained now to be ascended only the principal peak; the one designated in Winthrop's Journal by the name of the *sugar-loaf*, and in Belknap's New Hampshire by the name of Mount Washington. This we accomplished in half an hour, by climbing the ridge to the north of it, and walking on this ridge to the summit.

If the traveller could be transported at once to the top of this mountain, from the country below, he would no doubt be astonished and delighted at the magnitude of his elevation, at the extent and variety of the surrounding scenery, and above all, by the huge and desolate pile of rocks, extending to a great distance

in every direction beneath him, and appearing to insulate him from the rest of the world. But the length and fatigue of the approach, the time occupied in the ascent, the gradual manner in which the prospect has been unfolding itself, are circumstances which leave less novelty to be enjoyed at the summit, than at first view of the subject would be expected.

The day of our visit was uncommonly fine; yet the atmosphere was hazy, and our view of remote objects was very indistinct. The Moosehillock, one of the highest mountains of New Hampshire, situated in Coventry, near the Connecticut, was visible on the south. The Kyarsarge, Double-headed Mountains, and several others, were in full view at the east. The country around, in almost every direction, is uneven and mountainous. Its appearance is described by Josselyn, in his "Rarities of New England," published in 1672, who says that the country beyond the mountains to the northward "is daunting terrible, being full of rocky hills, as thick as mole-hills in a meadow, and clothed with infinite thick woods *."

Our anticipations were not realised, in regard to several phænomena we had been taught to expect at the summit. The state of the air was mild and temperate; so that the over coats which we carried up in expectation of extreme cold, were left at the foot of the last ascent. The thermometer stood at 57 Fahr. on the summit at twelve o'clock, and on the same day at Conway, twenty-five miles distant on the plain below, it was at 80. The snow lay in patches of an acre in extent upon the sides, but appeared to be rapidly dissolving. We were not conscious of any material alteration in the density of the atmosphere, as neither sound nor respiration was perceptibly impeded. Instead of an absence from these barren regions, of animal and vegetable life, we found a multitude of insects buzzing around the highest rocks, every stone was covered with lichens, and some plants were in flower in the crevices within a few feet of the summit.

The ascent from our encampment at the mouth of New river, including stops, had employed us six hours and a half. The descent from the summit to the same place occupied about five

* Messrs. J.W. and F.Boott, who have visited the mountain since, and found the atmosphere very clear on the summit at half past 7 A.M. have favoured me with the following bearings of objects in sight. The sea, supposed near Portland, S. E. by E.—Lake Winipisseogee, S.S.W.—A long hill, having an eminence at each extremity, said by the guide to be the highest in Vermont, W. by S. a little S.—Sebago Lake, S. E. ½ E. MacMillan's Inn, Conway, S. by E. ½ E.—The second highest summit of the White Hills, N.N.E. by E. This summit is separated from the one called Mount Washington by a gulf opening eastwardly. It is very lofty, falling but little below a horizontal line obtained by a level on the former place.

hours.

hours. We left on the mountain our names and the date, inclosed in a bottle, and cemented to the highest rock *.

Height of the White Mountains.

The great distance at which these mountains are visible, and the apparent length of their ascent, have led to estimates of their height considerably exceeding the probable truth. The Rev. Dr. Cutler, who twice visited them, and took barometrical observations, computes the height in round numbers at 10,000 feet above the level of the sea. Dr. Belknap, in his History of New Hampshire, is persuaded that this computation is too moderate, and that subsequent calculations will make the height even greater. Mr. Bowditch has published in the Transactions of the American Academy a logarithmic calculation, founded on the barometer as observed by Dr. Cutler and Professor Peck, in 1804, which gives them an elevation of 7,055.

Capt. Partridge, an engineer in the United States' service, visited the mountains some years since, and took barometrical observations on several of the principal peaks. His observations, now in possession of Professor Farrar at the university, give to the highest summit an elevation of only 6,103 feet.

A mountain barometer, of Englefield's construction, carried by Mr. Gray of our party, stood on the summit at noon at 24,23; the accompanying thermometer being at 57. At the same day, at Cambridge, the barometer stood at 29,95, and the thermometer at 76. This difference of the barometer, after making the necessary corrections for temperature, and variation in the surface of the cistern, would give, according to Sir H. C. Englefield's formula, a difference of 6,230 feet in the altitude of the two places. A logarithmic calculation was made, from the same data, by Professor Farrar, which resulted in a difference of 6,194 feet. This number being added to thirty-one feet, the height of Cambridge above the sea, will give 6,225 feet, which may be assumed as the probable height of the White Hills above the waters of the ocean.

In favour of the correctness of the observations on which this

---

* Parce, viator,
cui fulmina parcent.—
Hoc fragile monumentum
Lemuel Shaw,
Nathaniel Tucker,
Jacob Bigelow,
Franciscus C. Gray,
Franciscus Boott,
Bostonienses;
Die Julii 2do, A. D. 1816,
Monte Agiocochook superato,
hic reliquerunt.

computation

computation is founded, it may be observed, that the barometer employed was of the most approved and modern construction, being guarded against accidents with an express view to its use in expeditions of this sort; that it went and returned without injury; and at the end of the journey agreed with other instruments at the university, precisely as it had done before its removal.

In confirmation of the present estimate, it may also be observed, that a geometrical admeasurement, taken by Dr. Shattuck and others from the plain in front of Rosebrooks house, gave to the summit an elevation of 4,620 feet above that place. This being added to 1,648, the barometrical height of Rosebrooks above the sea, will give a total of 6,268 feet, differing only forty-three feet from our estimate.

W. Maclure, Esq. author of the geological map of the United States, informs me, that from two geometrical admeasurements made some years since on the eastern and western sides of the mountain, he arrived at results nearly similar.

Minerals.

The White Mountains when viewed from the westward, present a long ridge bounded by an undulating or serpentine line. On a near view, the outline is found to be notched and ragged, but wholly destitute of sharp cliffs and needles, or sudden perpendicular eminences. When the mountain is ascended, its uppermost or bald portion, 1,800 feet in height, is found to consist wholly of a loose, irregular, disconnected heap of rocks, of all shapes and dimensions, from one to thirty feet in diameter, lying confusedly one above another, but all resting firmly in their places, having found situations where they can resist the torrents, that roll over them, in descending the sides of the mountain at certain seasons of the year. These rocks are of gneiss and micaceous schistus, or rather of an intermediate substance between the two, approaching sometimes the one and sometimes the other. The mica is abundant and brilliant, but its stratification is uneven and irregular, and often interrupted by thin strata of quartz. Owing to the irregular position of the rocks, their strata are found resting in every possible direction. Large veins of quartz very frequently traverse them, and specimens of pure mica may occasionally be obtained, the plates of which are several inches in diameter. There is nothing in the colour of the rocks which can in any way account for the white appearance of the mountains, since they are uniformly incrusted with dark-gray lichens, which give them an almost blackish appearance. Their distant white appearance can only be accounted for by the presence of snow, which covers the summits for two-thirds or more of

of the year. In summer, their remote appearance is blue, like that of other distant objects.

In the middle and lower parts of the mountain, the character of micaceous slate, which in the course we took appeared to be the predominant constituent of the mountain, is more perfectly formed. The strata are remarkably smooth and even, and their fissure presents the most brilliant silvery lustre. The bed of the cascade at New River was principally of this material, intersected by thick veins of quartz, in which were contained large crystals of schorl. The pebbles in the streams were chiefly of micaceous slate, and occasionally of gneiss, of granite, and of pure white quartz. We also met with hornblende containing traces of carbonate of lime.

The object of most of our party being botanical, and our course generally rapid, the observations and collections we were able to make in mineralogy were necessarily limited. George Gibbs, Esq. who has twice ascended the mountain on different sides, with a view to the examination of its geology, has favoured me with the following remarks made by him at the time.—In some places where the geology of the mountain was exposed, he found the lower strata of greenstone and greenstone slate, with some granite. Higher up, granite and gneiss prevailed. The greenstone is fine-grained, containing pyrites. The greenstone slate contains actinote. The granite contains emerald, tourmaline, white quartz and feldspar, white and reddish mica, and garnets of different sizes. The granite is distinctly stratified. The strata of these rocks are from six inches to many feet in thickness, the granite being thickest, generally two or three feet. The dip of the strata is small and *from* the mountain. The rock, on the summit and for some hundred feet below, was gneiss, afterwards granite prevailed. Near the Notch Col. Gibbs observed rocks of coarse reddish jasper and porphyry, and obtained from the inhabitants specimens of fluor spar and magnetic iron ore.

Plants.

The vegetation of the White Hills has been divided with propriety into three zones. 1. That of the common forest trees; 2. that of dwarf evergreens; and, 3. that of alpine plants.

The woods which extend from the base up the sides to the height of about 4,000 feet from the sea, consist of the rock-maple (*Acer saccharinum*), which is the most abundant tree, the red maple (*Acer rubrum*), the silver fir (*Pinus balsamea*), the hemlock (*Pinus Canadensis*), the black and white spruce (*Pinus nigra & alba*), the white pine (*Pinus Strobus*), the beech (*Fagus ferruginea*), the black, yellow, and white birch (*Betula lenta,*

lenta, lutea & papyracea). The undergrowth was composed principally of the *Viburnum lantanoides*, the *Acer montanum* and *striatum*, and *Sorbus Americana*. Under our feet was the *Oxalis Acetosella* beyond every other species of plant, *Dracena borealis*, *Cornus Canadensis*, *Gaultheria hispidula*, &c.

Where the common forest trees terminate, the second zone of the mountain immediately commences, the line betweeen them being very distinctly drawn. This region consists of a belt of the black spruce and silver fir, rising to the height of seven or eight feet, and putting out long, firm, horizontal, or depending branches, so that each tree covers a considerable extent of ground. This mode of growth may be ascribed to two causes: 1. The great length of time that the snow rests upon them, weighing down their branches, and confining them in an horizontal direction. 2. The extreme cold which probably prevails here in winter, and which is destructive to all vegetation that is not secured by being buried under the snow. Upon the ground under these evergreen trees there were but few other vegetables. The only plants which I recollect in flower were the *Houstonia cœrulea* uncommonly large, and *Cornus Canadensis*.

Above the zone of firs, which terminates as abruptly as it began, is a third or bald region wholly destitute of any growth of wood. The predominance of rocks on this portion leaves but a scanty surface covered with soil capable of giving root to vegetation; yet to the botanist this is by far the most interesting part of the mountain. Many of the plants of this region are rare, and not to be found in the region below. They are for the most part natives of cold climates and situations, such as are found in high latitudes or at great elevations. Among them are natives of Siberia, of Lapland, of Greenland and Labrador. Vegetables of this race, usually known by the name of Alpine plants, have always been found difficult of cultivation. They are impatient of drought, and of both the extremes of heat and cold. During the severity of the winter, in their native situations they are preserved from injury by the great depth of snow under which they are covered, which secures them from the inclemency of the air, while they partake the temperature of the earth below them. When the snow leaves them, which frequently does not happen till the middle of summer, they instantly shoot up with a vigour proportionate to the length of time they have been dormant; rapidly unfold their flowers, and mature their fruits; and having run through the whole course of their vegetation in a few weeks, are again ready to be entombed for the rest of the year under their accustomed covering of snow. These plants, notwithstanding the high and barren elevations at which they frequently grow, do not suffer for want of moisture,

being

being constantly irrigated by the clouds which embrace them, and by the trickling of water over their roots from the eminences above.

The following list contains most of the plants, which we found on the uppermost or bald portion of the mountain. For a considerable increase of the collection, I am indebted to my friend Mr. F. Boott, whose botanical zeal induced him to undertake a second visit to the summit in August.

	In flower.
Aira melicoides. *Mx.*	Aug. 25.
Arenaria glabra. *Mx.*	Aug. 25.
Arenaria *seu* Stellaria—*caule anguloso; foliis oblongis, acutis, enervibus; pedunculis solitariis elongatis; floribus apetalis.*	Aug. 25.
Azalea Lapponica. *L.*	July 2.
Azalea procumbens. *L.*	July 2.
Bartsia pallida. *L.*	August 25.
Betula lutea. *Mx.* nana.	
Campanula rotundifolia. *L.*	August 25.
Cardamine rotundifolia. *Mx.*	July 2.
Carex curta. *Willd.*	August 25.
Carex cæspitosa. *L.*	July and August.
Coptis trifolia. *Salisb.*	July 2.
Cornus Canadensis. *L.*	July 2.
Diapensia Lapponica. *L.*	July and August.
Epilobium alpinum. *L.*	August 25.
Empetrum nigrum. *L.*	August 25.
Geum Peckii. *Pursh.*	July and August.
Holcus monticola—*glumis trifloris, hermaphrodito intermedio diandro, masculis lateralibus triandris, valvula exteriore dorso aristata.*	July 2.
Houstonia cœrulea. *L.*	July 2.
Juncus melanocarpus. *Mx.*	July 2.
Juncus spicatus. *L.*	August 25.
Kalmia glauca. *L.*	July 2.
Ledum latifolium. *Ait.*	July and August.
Lycopodium lucidulum. *Mx.*	
Melica triflora—*villosa, panicula coarctata, glumis trifloris, corpusculo accessorio; flosculis aristatis.*	August.
Menziesia cœrulea. *Swz.* (Erica, *Willd.*)	July 2.
Oxycoccus vulgaris. *Pers.*	July and August.
Pinus nigra. *L.* nana.	
Pinus balsamea. *L.* nana.	
Polygonum viviparum. *Willd.*	August 25.
Potentilla tridentata. *Ait.*	July.
Rubus saxatilis. *L.*	August 25.

Salix

In flower.

Salix repens. *Willd.* .. .. July 2.

Scirpus obtusus—*culmo tereti, nudo, monostachyo; spica lanceolata, squamis apice carnosis, obtusis.* July.

Scirpus bracteatus—*Culmo tereti, monostachyo; spica ovata, acuta, bracteis involucrata; flosculis monandris.* .. .. August.

Spiræa alba. *Ehr.* .. .. .. August 25.

Solidago multiradiata. *Ait.* .. .. August 24.

Sorbus Americana. *Willd.* nana.

Vaccinium tenellum. *Ait.* .. .. .. July 2.

Vaccinium gaultherioides—*prostratum, foliis obovatis, integris; floribus subsolitariis; baccis oblongis, stylo coronatis.* .. .. .. July 2.

Veratrum viride ? Wild. *s. fl.*

Lichen velleus, rangiferinus, pyxidatus, cocciferus, Islandicus, cornutus, et alii plures.

Indeterminatæ Salix 1. Poa 1. Menziesia ? 1.*

The vegetation in spots extended quite to the top of the mountain. *Diapensia Lapponica* and *Lycopodium lucidulum*, the former in full flower, were growing within six feet of the summit. All the rocks were incrusted with lichens, among which *L. velleus* is the one which predominates, and contributes essentially to the dark gray appearance of the mountain.

In the foregoing list of vegetables, it will be seen that a considerable number of species are natives of Europe as well as of this country. A question of some interest has arisen—whether any plants are originally common to both continents†, and whether those species which approach each other so nearly in their external characters, as to be known at present by the same names, are in reality the same species. The analogy of the animal kingdom seems to favour the negative of this question. Baron Humboldt has asserted, upon the highest authorities, that no quadruped or terrestrial bird, and even no reptile or insect, has been found common to the equinoctial regions of the old and new world. In like manner he affirms that the phanerogamous plants, which have been recognised as natives of the tropical regions of both continents, are extremely few. In the temperate zones, the number of American plants which wear European names is continually diminishing in books. The separation of

* Specimens of the plants were sent to the Right Hon. Sir Joseph Banks; and Mr. Boott (we understand) has pointed out the following corrections, the result of a hasty examination of the plants which he made with Mr. Brown. *Lycopodium lucidulum* is *H. Selago* (Smith's *Flora Britannica*).—*Holcus monticola* is *H. alpinus* (Wald. *Flora Lapponica.*)—*Scirpus bracteatus* is *Juncus trifidus* (Aiton's *Flora*).—Edit.

† Humboldt.—Memoir on the Distribution of Vegetable Forms.

them

them has in some instances been carried further than a strict adherence to the present grounds of botanical distinction will justify. Yet there still remain species wholly agreeing in their botanical characters, but sufficiently differing in their qualities, places of growth, times of flowering, &c. to render it not improbable that they are distinct. A species of *Æthusa* grows about Boston, which externally bears the strictest comparison with *Æthusa Cynapium* of Europe. It is however altogether destitute of the nauseous or garlic taste for which that plant is noted. *Menyanthes trifoliata* in New-England flowers a month earlier than in Great Britain, though our seasons are perhaps always more backward. Botanists have not yet distinguished the chesnut-tree of this country from that of Europe, although its wood is weak and brittle, and never used, as in Europe, for hoops and other purposes where strength and tenacity are required. On grounds like the foregoing a great number of vegetables which have not emigrated to us since the discovery of America, and which are not found far to the north of us, may be suspected of being really distinct in nature from those which nearly resemble them in Europe, and are known by the same names*.

But as we approach toward the north, and arrive in high latitudes, the probability of finding plants identically the same is greatly increased. About the arctic circle, the two continents approach each other so nearly, and are so connected by ice during part of the year, that they may, as far as botany is concerned, be considered the same country. The same plants may be equally disseminated on both, and these may extend as far toward the south as the general coldness of the climate suited to their constitution continues. Beyond this they may for some distance be found in alpine situations on the tops of the highest

* Still we should strictly beware of hastily changing names, and establishing new species on slight or doubtful distinctions. Botany, at present, knows no other mode of distinguishing plants, than that by their external forms, and to this, in the present state of the science, we must rigidly adhere. If an American plant cannot readily be distinguished from an European, by a clear specific character, no harm can ensue, and much confusion may be avoided, by suffering it to remain as a variety, under that same specific name. A zeal for the discovery and establishment of new species, however laudable in its general object, has been productive of much mischief to the botany of this country. We have had many specific names founded in varieties, and many accompanied with hasty and imperfect descriptions, which leave it doubtful whether they refer to species or varieties. Different botanists, without communication or intercourse with each other, have described the same plants under different names, and different plants under the same names, in various parts of the country. There is at present no greater obstacle to the progress of botany here, than the load of uncertain synonyms, doubtful species, and superfluous names, with which many of our best books are encumbered.

mountains.

mountains. There are also plants of such versatility of constitution, that they bear all the varieties of climate from Hudson's Bay to Virginia and Carolina. Such plants may well be common to the two continents.

Animals.

The unsettled state of the country for some distance around these mountains, the many recesses and solitudes which they possess, that are rarely visited by man, has rendered them still a resort for many of the original animals of the continent, whose species have nearly disappeared from the more inhabited parts. The moose (*Cervus alces ?*) still resides here; and we were told that upon the Pliny mountains, about twenty miles to the N. W. some of these animals are killed in the course of every winter. The bear (*Ursus Americanus*) inhabits the woods about the base and sides of the mountain, where he is not unfrequently met with *. The wolves (*Canis Lupus*) being gregarious, move in troops, and are said to visit this part of the country once in three or four years. Several of them were killed last winter in Eaton, a town adjoining the mountains. The wolverene (*Ursus luscus*), raccoon (*Ursus Lotor*), porcupine (*Hystrix dorsata*) and sable, the two latter in considerable numbers, are found in various parts of the forests. The wild cat (*Felis montana ?*) is occasionally killed here. The catamount (*Felis concolor* s. *Couguar*) is at the present day seldom heard of.

Of birds we saw but few. Most of our migratory land birds, choosing to share with man the fruits of his cultivation, are more frequently found about the abodes of civilization than in the solitude of the forest. In Bretton woods several woodpeckers were shot by our party, all of them very beautiful species; and among the rest *Picus tridactylus*, remarkably distinguished from the rest of his family by the number of his toes. The partridge (*Tetrao Umbellus*) we frequently scared. This bird, as well as a species of plover or of *Tringa*, has been seen in the upper or bald part of the mountain.

We were told by the people in Bartlett and Conway, that the rattlesnake (*Crotalus horridus*) infests the rocks and sides of the hills in great numbers, and that twenty of these reptiles had been killed in a day. They even approach dwelling-houses, at the doors of which they have been killed. The inhabitants regard them with little apprehension, since they are represented as slow and clumsy in their motions, and as always giving notice on being approached, by a loud and long-continued rattle, resembling very much the singing of a locust. We saw none of

* Our guide informed us that one had been in his inclosures the week preceding our visit.

these

these serpents, and heard of no injury sustained by any one from their bite.

The insects which we observed at the top of the mountain were as numerous and various as in any place below. Among them were species of *Phalæna*, *Cerambyx*, *Coccinella*, *Buprestis*, *Cimex*, and *Tenthredo*. The most splendid of our native butterflies (*Papilio Turnus?*) was fluttering near us while we remained on the summit.

VIII. *Experiments on various refractory Substances by means of the Oxi-hydrogen Blow-pipe.* By J. Murray, *Esq.*

To Mr. Tilloch.

Sir,—I feel happy in being able to resume the detail of experiments made with Newman's blow-pipe, an instrument of no mean importance to the chemist and mineralogist.—The application of an explosive mixture of the gaseous constituents of water, in a condensed form, for the obtainment of an exalted temperature, marks an extraordinary period in the annals of science. The chain of brilliant results which has characterized its application by Dr. Clarke* gives to the machine a most imposing and interesting form. The scientific chemist will appreciate its value, and duly acknowledge his thanks to Dr. Clarke, who has first pointed out its control over the most refractory constituents of the globe, in that masterly manner of which his results are evidence. Not satisfied with the exhibition of these beautiful phænomena, he has succeeded in rendering the instrument *perfectly secure* against explosion. The substitution of a little oil instead of water, in the cell of the reservoir, is all the requirement. Dr. Clarke has been good enough to communicate this important fact to me; and I am glad that the event of my experiments since that period, has fully justified the conclusion. Much praise is also due to Mr. Newman of Lisle-street, for the ingenuity he has displayed in its structure, and his unwearied efforts to render it safe. I may here state that Geo. Rennie, esq. joined me in the experiments which I shall now describe; and though a second explosion has occurred in the course of our manipulations, it has served but to redouble our exertions, and impart a new impulse, and excite a livelier interest.

1. Amianthus was fused into a *dark bead*, exhibiting a brilliant light during fusion.

2. Petuntze, one of the constituents of Indian porcelain, exhibited a *most splendid* light, and fused.

* Detailed in Dr. Thomson's Annals of Philosophy.

3. A Si-

3. A Siberian Beryl was fused, with an exhibition of most *brilliant* light.

4. An Oriental Topaz rapidly fused, with beautiful light.

5. Some dust, which fell from the atmosphere at *Messina*, evidently volcanic, and under the lens of the microscope exhibiting the semblance of *minute cinders*, part of which was attracted by the magnet, fused into *dark glassy beads*.

6. Brown Pumice Stone was fused into a *dark brownish glass*, approximating to obsidian; and I have do doubt but that they originate in the same source.

7. Red Coral exhibited a painfully intense light, and fell into a white powder, *exhaling a marine odour*.

8. White Pumice Stone fused into a *semi-transparent glass*.

9. Diamond Powder, mixed with olive oil, being exposed to the full blast of the ignited gases, in the bowl of a tobacco-pipe, *exhibited a jet of brilliant stars*.

10. Gold Wire entered into *tranquil fusion*.

11. Wood Tin reduced to a powder, and mixed up with oil, placed in a nidus of charcoal, was reduced, and *brilliant metallic globules*, accompanied during reduction with a *violet* flame, were obtained. This substance was the most refractory in our experiments.

12. The Jacinth, from Ceylon, was instantly *fused into a globule*.

13. Obsidian immediately *melted*.

14. Black Oxide of Manganese was speedily reduced. *Manganese melted*, but was somewhat refractory.

15. Cologne Basalt was fused into a dark gray mass.

16. Vesuvian, resembling a semivitrified cinder, was fused into a *black glass*.

17. Pozzolano was fused into a *dark mass*, and became *powerfully attracted by the magnet after fusion*. It had no attraction whatever, previously.

18. Iron Ore from *Bradford Shey* iron-works, containing nearly 70 per cent. of iron, was *fused* and *reduced*.

19. A Steel File burnt with *uncommon splendour*, and *fused instantly*.

20. Two pieces of a Tobacco-Pipe were fused into glass, and *agglutinated together*.

21. Steatite (Magnesian) was fused with vivid light, and two pieces *agglutinated*.

22. A piece of Talc *fused* with *intense splendour*, accompanied with *a beautiful iris*.

23. Grains of *Platinum, Gold*, &c. instantly fused into a globule.

24. Smalt

24. SMALT from the King of France's *porcelain manufactory* at *Sevres* fused into a mass, and *resigned its colour.*

25. The OPAL decrepitated violently.

26. The CALCEDONY from *Iceland* fused with intense light into a *fine white frothy enamel.*

I have thus selected a few of what I consider the most interesting experiments, and shall be happy in resuming my account of further trials.

Before this instrument the most unyielding bodies forgo their powers of cohesion, and enter into rapid fusion. Its energy of temperature is described in one of the lines of Pope—for before it

"Rocks fall to dust, and mountains melt away."

In the course of our experiments we have met with *no substance* sufficiently refractory to resist its intensity of ignition. Some of these experiments have been detailed by Dr. Clarke; but I deemed it best to submit an unbroken series: and such of them as have been described by Professor Clarke will find a full corroboration in our repetition. We are even now only upon the threshold of discovery. The rays of chemical science are bounded by no horizon:—mightier wonders remain to be revealed.

I am, with very great respect, sir,
Your obliged and faithful servant,

London, January 22, 1817. J. MURRAY.

P. S. Two EMERALDS were fused into ONE MASS, exhibiting, during fusion, a light too vivid for the eye to *endure without suffering.* The beautiful globule thence obtained had become *limpid* and colourless, with the exception of a band or zone of enamel, like a horizon, circling the spherical mass. The specimen is in possession of Geo. Rennie, Esq. who, as well as in the other experiments, joined me in this.

IX. *On the ancient Names for Colour, particularly as applied to the Colour of the Light of Stars.* *By* T. FORSTER, *Esq.*

To Mr. Tilloch.

SIR, — HAVING lately met with several treatises on the mutations of colour in the light of the fixed stars, in which this fact has been attempted to be proved by citations from the works of the ancient poets and others, who have described the colour of particular stars now evidently not answering the same description; and having myself made some observations on the colour of stars, I was led to refer back to the ancient au-

thorities themselves, and was consequently induced thereby to enter more at large into the real import of the names used by the Roman and Greek writers for *colours*. And I am convinced that no accurate argument can be drawn from the Classical writers respecting the real colours of stars, or any other natural bodies, from the very vague and indefinite meaning of the names of colours themselves, and also from the excessive latitude given by poetic writers to the signification of words. I would, however, make an exception of such cases as those wherein two particular colours are contrasted to each other in the same passage.

To apply this observation to our subject: Though the observations of Tycho Brahe, and other more modern astronomers, relative to the light of the stars, are valuable, being written at a time when, philosophical Latin having become the medium of communicating science, terms were used in a more definite sense; yet I should suspect the accuracy of any arguments drawn from the early Greek and Roman poets, in which the colour of the starlight is alluded to; making an exception only of such passages as contrasted strikingly opposite colours.

I shall offer a few examples to illustrate this position; and conclude with referring to the etymological import of many words designating colours.

I. *Passages wherein two or more colours are contrasted and distinguished, but which still do not accurately define the precise tint.*

The ancients have in some instances used words for colour in a sense hardly to be mistaken, by employing them to contrast two phænomena whose difference of colour is marked and well known everywhere, and which we have no reason to think changes from time to time. Instances are to be met with, in which the colours produced by refraction of the light of the heavenly bodies constitute examples.

Ovid, in Metamorph. xv. 192, uses *rubere*, to redden, for the red colour of the sun near the horizon, produced by denser refraction; and contrasts it with the term *candidus*, applied to the sun in his greatest altitude; whereas from a lesser degree of refraction it approaches nearer to *white*. Thus,

> "Ipse Dei clypeus, terrâ cum tollitur imâ,
> Mane *rubet*: terrâque rubet, cum conditur imâ,
> *Candidus* in summo est. Melior natura quòd illic
> Ætheris est, terræque procul contagia vitat."

Virgil, in Georg. i. 431, among the signs of wind, evidently marks the *red* colour of the moon:

> "At, si virgineum suffuderit ore *ruborem*,
> Ventus erit: vento semper *rubet* aurea Phœbe."

He

He shortly afterwards contrasts this red colour with *pura*, or *white*, that is free from coloration, when, before fine weather, the moon,

"*Pura*, neque obtusis per cœlum cornibus ibit."

Horace has the same expression for the pure light of the moon, in Carm. ii. v. 20.

"Ut *pura* nocturno renidet
Luna mari."

Aratus also, in Dios. 53, evidently contrasts the red with the white colour, in the following lines on the presages of weather:

Λεπτη μεν καθαρη τε περι τριτον ημαρ εουσα
Ευδιος μεν ειη· λεπτη δε και ευ μαλ' ερευθυς
Πνευματιη.

Again, speaking of the more certain prognostic of rain in proportion as the circles called *halos* are more reddened,—a fact well known to modern meteorologists,—he says:

........ολος περι κυκλος ελισση
Παντη ερευθομενος, μαλα μεν τοτε χειμερος ειη,
Μειζονι δ'αν χειμωνι πυρωτερα φοινισσοιτο.

Theophrastus, in *Sign. Temp.* contrasts the blackish colour of the sun and moon, by the interposition of an obscure cloud, as a sign of rain, with the red colour which is a forerunner of wind. *Εστι δε σημεια ηλιω και σεληνη, τα μεν μελανα υδατος, τα δε ερυθρα πνευματος.* Consult also *Plin. Hist. Nat.* xviii. 35. The learned Scapula in *Lex. Gr.* observes of the word *μελας*, *black: vox sæpe pro horrendo aut obscuro posita, quia talia sunt atra**. Thus the *confused* or the *black* appearance might constitute the contrast to the red one, as above cited.

II. *Passages wherein a most extended and undefinable signification is given to the names of colours by the ancients.*

A proof of the very indefinite import of names for colours among the ancients may be drawn, 1st, from their incongruity with the known colours of the substances to which they are applied, if taken in the strict sense: and 2dly, from examples in which the same term is applied to bodies of a known difference of colour.—A few examples will suffice, which happen to be in my mind, and will lead to a more extensive inquiry.

Purpureus is applied,

1. To the rose, in which it approaches the nearest to our modern idea of *purple*;—thus in Virgil, c. 14.

"Rosa *purpurea*."

* If the reader wishes to pursue this subject further, I have put a long list of references in the notes to a late edition of the Diosemeia of Aratus, p. 11, and sequel.

 2. To

2. To the colour of the violet; as in *Virg. Georg.* iii.

> "Violæ sublucet *purpura* nigræ."

3. To the narcissus, as in *Virg. Ecl.* v. 38.

> "Pro molli viola, pro *purpureo* narcisso."

4. To the colour of the sea, as in *Virg. Georg.* iv. 373,—speaking of the Po, or Eridanus:

> "——— quo non per pinguia culta
> In mare *purpureum* violentior influit amnis."

5. To the colour which is given to the green waves of the sea (*virides undæ*) by the rough wind; as

> "Spiritus Eurorum virides quum *purpurat* undas."

In which case *purpurat* may mean, 1st, that it gives the sea that purple colour sometimes occurring in storms; or, 2dly, that it causes the brilliant white or silver colour of the frothy spumes of the rough waves in the sun: the last sense is most likely, as purple is applied.

6. To the brilliancy of the sun and other luminous bodies in general; as

> "*Purpureos* inter soles, et candida Lunæ
> Sydera."

The word πορφυρεος has in Greek a signification almost as unlimited.

7. To flowers in general, as if it signified only brilliancy, from the gaudy contrast of flowers to their leaves.

CANDIDUS is applied,

1. To things fair or beautiful in the most general acceptation, as to the constellation *Taurus*.

> "*Candidus* auratis aperit cum cornibus annum,
> Taurus," &c.——*Virg. Georg.*

2. To the sun when high, either from its brightness or whiteness in comparison with his disk in a horizontal position,

> "*Candidus* in summo est," &c.——*Ovid.*

Also, Solis *candor* frequently for the sun's bright light.

The word comes from *candeo*, to shine, and has no limited meaning, as to colour, whatever.

The word CŒRULEUS seems to have been derived from κοιλος. It is applied,

1. To the snake, perhaps from the blue colour of some particular snake.
2. To the sea—*Cœrula* verrunt (remigio). See also many passages wherein the ocean seems to be called *cœruleus*, *viridis*, and *purpureus*, almost indifferently, as it accorded

accorded with versification in poetry, or avoided tautology. —I may add, that *Albus* is the most restricted in its signification. *Niger* as extended as any. *Argenteus* and *Aureus* are again more limited in their meaning, as are in general the colours taken from metals.

FLAVUS is put,

1. For red hair, as *flava* Minerva, &c.
2. For many flowers of different colours, for crimson, &c. And numerous other substances might be added, were it not for swelling this paper.

RUBER is applied variously:

1. To the hyacinth*, as *Virg. Eclog.*

   "Suave *rubens* hyacinthus."

2. To the red colour of the heavenly bodies before wind, as above quoted:

   "Vento *rubet* aurea Phœbe," &c.

3. To the moon itself, although her colour is so often contrasted with *red*, and called *pallida*, *alba*, *candida*, &c.; consequently cannot ever be depended on as representing the *red* colour of any fixed star, called by the ancients *rubra*.

Festus Avienus in the *Anthol. Vet. Lat.* says,

> ". . . . . reparatum Cynthia format
> Lucis honore jubar, curvatis cornibus arcus
> Quod de fratre *rubet*."

There are so many other instances where *rubere* is put for *nitere*, *splendescere*, &c. that I am convinced no argument can be drawn from its application to the light of stars by ancient poets. An ingenious paper on this subject, however, has been inserted in the Philosophical Transactions by Mr. Barker; but from an examination of the subject among the ancient writers, too long to be inserted in full here, I am induced to rely much less than he does on the particular signification of the ancient terms for colour.

Similar observations apply to *πορφυρεος*, *ξανθος*, *λευκος*, *cœruleus*, *fulvus*, *luteus*, *albus*, *niger*, &c.; and to the words for colours in other languages. In short, in the progress of science, while literature was emerging from the dark ages, the names of colours, like other words, were derived from reference to particular coloured bodies: but as there were

* It is not quite certain what flowers the Hyacinth and Narcissus of the ancients were. Our present narcissus has no properties from which it could be derived, as it evidently is from ναρκιω.

not names enough to express all the tints of nature, their signification was widely extended, particularly by the license of the ancient poets; and it is only by modern philosophers, as science advanced, that they have become more determined in their signification.

In a former paper in the Philosophical Magazine* I have given a series of the etymological derivations of the names for colours in our language, where by reference to old books, and by the facility of etymology, in English, we can trace them to their primitive meaning in the mother tongue; and I have proposed a new Nomenclature for colours, which shall express the proportion of primitive colours which compose the beautiful and extensively varied tints by which the surface of the globe is everywhere ornamented. And I have added these observations, on the application of the names for colour to the illustration of change of colours of the fixed stars, with a view that those who have more classical

* See Phil. Mag. Nov. 1813, vol. xlii. p. 327; also for Aug. 1813, vol. xlii. p. 119.

YELLOW.—This word is derived from the Anglo-Saxon verb ʒeælʒan, *accendere* (to inflame,) and signifies the colour of flame. In like manner the Latin *flammeus,* as well as *flavus,* come from φλεγμα, flame, from φλεγειν.

RED.—The etymology of the word seems doubtful. Horne Tooke has omitted it in his etymological account of colours in the Diversions of Purley. I suspect, however, it may have some connection with the word *ray,* and expresses the colour of the sun's rays.

BLUE.—This word seems to come from blopan, *florere* (to blow as a flower does), and signifies the colour of flowers;—certainly the most indefinite of all our names for colours, like the Greek ξανθος, from εξ and ανθος, i. e. *color e floribus.*

GREEN is derived from the Anglo-Saxon verb ʒꞃennian, *virescere.* In like manner the Latin *virere* gave the adjective *viridis.*

PURPLE is commonly used in modern times for the compound of red and blue, is derived from the Latin *purpureus,* and signifies only *flame coloured;* from πῦρ, *fire.*

BROWN is a corruption of the past participle of the Anglo-Saxon verb bꞃennan, *urere* (to burn), and signifies the colour of burnt substances; having etymologically no distinction between it and ash colour. In like manner the Latin *fuscus* comes from φωσκειν, *ustulare,* as noticed by Tooke; and has the same real meaning, as well as the same application, as *brown.* Query, Whence come *fulvus* and *aquilus?*

WHITE comes from the Mœso Gothick ΘΛΦGΛN, *Spumare.* Our word *Gray* is derived from ʒeꞃeʒnian, *inficere,* meaning the colour of tainted, infused, or damaged articles, and is most properly used when applied to mixtures, which appear as if tainted or tinged with foreign colours, as the salt and pepper mixtures, &c.

The dilutions of yellow by white are called *straw colour.*

BLACK has probably the same root as "bleak," perhaps from blæcan, and signifies deprived of colour.

reading

reading and opportunities of investigation, may see one source from whence we can detect the fallacious evidence of the old poetic writings in matters of philosophy.

I am, sir, yours, &c.

Clapton, Dec. 9, 1816. THOMAS FORSTER.

X. *Combination of the Electric Column, the Thermometer, Barometer, and Hygrometer in one Instrument, for Electro-atmospherical Researches. By* A CORRESPONDENT.

To Mr. Tilloch.

SIR, — THE electric column of De Luc promises to be not only an interesting but also a useful apparatus, as its powers seem to be influenced by the electrical state of the atmosphere. It has occurred to me, that a combination which would at one view give all the different states of the atmosphere, would be very convenient in electro-atmospherical researches. If you should consider the following worthy a place among your more important communications, it is much at your service.

Yours, &c.

D.

I trust the plan will not require much explanation. It may be sufficient to say that A (Plate I. fig. 6) is a powerful column insulated on Mr. Singer's plan; C, a thermometer; and B, a barometer, which should have a brass or an ivory index with a gilt wire to form a communication with the bottom of the column; the parts E F may be covered with sealing-wax; G may be a small figure or a glass tube; D is the index of a catgut hygrometer stretched on small pulleys on the bottom of the stand, as is represented by H—I (fig. 7 and 8.)

XI. *Mr.* FAREY'S *Correction of his Remarks in our last Number, regarding a Geologist being attached to the Government Trigonometrical Establishment.*

To Mr. Tilloch.

SIR, — EVIDENT justice to the Parties who direct the Government Trigonometrical Survey of our Island, carrying on under the Board of Ordnance, and toward a highly respectable and able Mineralogist and Geological Investigator (the President this year of the Geological Society) requires of me, without delay, to

 apologize

apologize for, and correct the too hasty *doubts* which I have thrown, in pages 427 and 431 of your xlviiith volume, on the alledged fact, of Dr. MacCulloch (whose *chemical* services in the Board's Establishments are well known and appreciated) being associated with Col. Mudge and Capt. Colby in the Trig. Survey, for applying the principles of Geological investigation, to the ascertaining of the subficial structure of the vicinity of each Station therein.

Some of the gentlemen who honour me by their friendship, whom I had supposed *very likely to know* of any such association, or investigation being on foot, as I have mentioned, were, as I had the means of ascertaining (even since your last number appeared), *unacquainted therewith*, and that most others of my acquaintance were equally so, I had reasons to conclude: and besides, the notification I have thrown doubt upon, assumed, that Dr. M. would be "going from station to station *with*" the Trig. Surveyors, contrary to the fact. These circumstances misled me from the course I ought to have followed, of making direct inquiries, before so expressing myself in print.

I have sincere pleasure in adding, that I now understand, Dr. MacCulloch entered near three years ago on the task, taking the Country before him in such order, locally, as he judged best adapted to his investigations, of ascertaining the Geological facts regarding the vicinity of each Station. The great abilities displayed by Dr. M. in the many valuable Papers with which he has enriched the volumes of the Geological Transactions, and his known preference, to depending on his own personal *observation*, as to *identity or order of superposition* of the terrestrial Masses, to dependence on any *Geognostic Rules*, founded merely on the *Mineral characters of hand-specimens* collected (however numerous they may be), which seems so common among those, who would degrade *Geology* into a mere "branch of Mineralogy;" all those considerations excite in me great expectations, and a strong desire to see the results of Dr. M's labours towards the important objects, on which I have been solicitous for 15 years past, as explained in your last number. If you can oblige me by insertion at this very late period of the month I shall feel highly obliged, and am, sir,

Your obedient servant,

Howland-street, Jan. 26, 1817. JOHN FAREY Sen.

XII. *On*

XII. *On the indispensible Necessity of perfectly ventilating Coal-pits, and the Insufficiency and Danger of Safety-lamps, as Substitutes for such Ventilation.* By Mr. JOHN FAREY, Sen. *Mineral Surveyor.*

To Mr. Tilloch.

SIR, — As your correspondent, Mr. J. Murray, affects *now* great *moderation* in the discussions carrying on, regarding *safety-lamps* for coal-pits, and says, in your last number, p. 453, "It is painful to see the asperities of controversy" on this subject,—I think it but just, on the part of Mr. Longmire, myself, and others, who have been aspersed, to request the insertion of the following letter, which appeared in a newspaper printed at Carlisle, called the "Patriot," of February the 10th last:

"*To the Editor of the Carlisle Patriot.*

"SIR,—I confess I read in the *Old* Monthly Magazine of the 1st inst., with considerable surprise, and *no little indignation,* the attempt of Mr. Farey, senior, to damp the efforts of scientific men in the cause of humanity, and to interdict the continued use of safe-lamps. *He* would *hang* up the *safety-lamp like a bucket* for the extinguishment of flame, only to be used when *the house is on fire.* The safety-lamp ought 'to be hung up in the office of the overlooker, on the pit hill,' and to be used occasionally, forsooth; i. e. on the recommencement of labour on the Monday, or the like. This is *absolutely trifling with human existence*; it is playing at *the game of life and death.* I affirm that it is impossible to calculate on the explosive point, so as to take precautionary measures, ensuring safety; the disengagement of carbonated hydrogen may be instantaneous, and the explosive measure filled up on the moment. Breaking into old workings, for example, or excavating seams where the *blowers* are more prevalent, would render the *occasional* use of the safe-lamp a mere non-entity;—to act on Mr. Farey's *wise* suggestions, is neither more nor less than to *sport on the verge of a precipice.* Certainly, I admit with Mr. Farey, that the use of the safe-lamp ought not to relax our endeavours in the grand and primary object of a proper *ventilation*—but to say that its unintermitting use *would relax* such efforts, is to argue from the *abuse of a valuable discovery*, and is *illogical.* *Farey* would, it seems, only employ the lamp when it could be of *no use*: this is really a curious specimen of his *reasoning,* if reasoning it may be called. When the phænomena on the flame of a candle indicate that the explodible mixture has reached its *maximum*, the lamp of Sir H. Davy is of no service whatever, for it is instantly extinguished. Now, how must those who have laudably directed their labours

in

in this channel *think of Mr. Farey and his associates?* This is indeed a bad reception, and a sad return towards those who sacrifice their means, and direct their time and talents towards 'a consummation so devoutly to be wished.' Certainly, there is no remuneration but that wholesome return which springs up in the breast, when it approves the labour.

"It surely must be apparent to every one the least conversant with chemical philosophy, that the sparks from a *steel mill*, which serve to guide the miner in exploring the workings, can afford him no *certain security*. I confess that the recommendation of Sir H. Davy, for employing *red hot charcoal* (not exhibiting flame) when his lamp can no longer burn, was one which *startled me*. What is the consequence of such a substitute? An exchange of carbonated hydrogen for carbonic acid gas.—*Fire damp* is supplanted by *choke damp*—we escape Scylla to suffer by Charybdis. The question has somewhat of the complexion of the antidote proposed for accidents by fire in the theatres. The good people were calmed by the *comfortable assurance*, that in such an occurrence the managers could *drown* the pit in a few minutes! Thus, then, we want a lamp to guide us when such a desideratum is most needed. This is amply afforded by a proposition which I published in my 'Elements of Chemical Science,' in August last, for bringing the atmospheric air from the stratum of air contiguous to the floor of the mine, taking advantage of the ascending carburetted hydrogen:—this I proposed to do by a flexible tube. My latest improvement of the chimney was a spiral tube, having a central sphere. I have since applied a *small lever* with a *delicate ball* at each extremity—this operates in the hollow sphere, and so long as a free current is maintained, the ball hangs *in medio*; but should unforeseen circumstances, on which it is impossible to calculate, occasion the flame to ascend, the air which it propels before it, occasions the ball to rise, and shut the upper aperture; and again, should a current descend, the ball will, by the impulse, fall, and shut the under aperture. That the projection of this lamp rested with me in point of priority, I have clearly proved, *and not with Dr. Murray of Edinburgh*. I lay this claim, therefore, as the first who publicly proposed an air-tight lamp, to be fed by air brought from the floor of the mine. The mere aperture at top, instead of the chimney, as proposed by Dr. M., gives no assurance of security whatever. However convinced I may be of the certain safety of the spiral chimney, *independent of the lever*, still I have wished to make 'assurance doubly sure,' by providing against any possible casualty. My claims of this priority, and a description of a simple and efficacious air-tight stove for drawing off the carbonated hydrogen as it forms in mines, with an easy mode of getting

getting rid of the carbonic acid gas (choke damp), have been laid before the Society for Preventing Accidents in Mines, by Lord Percy, and the Royal Society of Edinburgh, by Mr. Neill, Secretary to the Wernerian Natural History Society. My remarks were before the public in August last, and *three months* ere this, in the hands of my printer: I had also communicated my sentiments hereon even long before, to my friend J. C. Gotch, esq. banker, Kettering. Far from underrating the value of Sir H. Davy's lamp, I think it deserves instant adoption, and to be continually used. These lamps may *go together*: the one is more convenient for general purposes, the other will act in cases where its fellow is of no use. Sir H. Davy's exertions in the cause of humanity deserve the eternal gratitude of that species of which he is an honour. I confess I see no force in the objections of Dr. Thomson to Sir H's safe lamp.

"If you will allow me, I shall in your next number give such cautions in the employment of charcoal, and in the occurrence of choke damp in cellars, wells, &c. and the means of subduing their effects, as may be useful.

"Carlisle, Feb. 8. 1816. J. MURRAY."

On this day, reperusing the letter in the *Monthly Magazine* which Mr. M. thus harshly and unfairly commented on, and also an earlier letter of mine in the same work, inserted also in your xlvth volume, p. 436—and contrasting these with the numerous letters, papers, and essays which have since appeared on the subject of inflammable gases in coal pits, I beg to say, that I do not observe a single fact or point of my arguments therein, which has yet been materially shaken, by any thing which has since transpired or been advanced. Indeed, Mr. M. alone seems to have had the hardihood, to attack either the matter or the manner of my communications; and in what way and manner he (who *now* affects moderation) did this, your readers will see above.

In arguing (Monthly Mag. Vol. xli. p. 33.) on the chief and peculiar defects of the Newcastle system of Collierying, viz. *the vastly too greatly extended Works to be ventilated by one up-cast Shaft*, I assumed only, with Mr. Buddle, that the "run of the air," or length of laterally air-tight gallery, which required to be constantly maintained, "*or imminent danger ensues*," sometimes exceeds thirty miles!; but on making inquiries, last summer, as I passed through, and made a short stay at Newcastle, I was then very credibly informed, that I had very greatly under-rated the extent of the *chief cause*, which had occasioned so much fuss about safety-lamps; for that forty, fifty, sixty, and even ninety miles of such air-courses were, or had lately been attempted to be maintained, between a down-cast and an up-cast shaft!!; and that in various parts of this vast single magazine of combustible

or

or explosive gas, almost proportionally large numbers (above what is usual in large Collieries any-where else) of Men and Boys, thoughtless of their danger, were daily employed.

Judge therefore, reader, who it is who are "sporting on the verge of a precipice," "trifling with human existence," "playing at the game of life and death," &c.: I, who have candidly pointed out the peculiar circumstances of management, which have led to the catastrophes which all deplore, and who have laboured to bring back these important Works, not to any new or theoretic point of improvement of my own pointing out, but merely to *the moderate extent of individual underground Works* *, *and great comparative security to the Men, which they had and enjoyed* 40 *or* 50 *years ago!*, and which the Colliers in nearly every other part of the British Empire still enjoy!; and long will continue to do so, I trust, from their Managers being fully aware, of *the perfect insufficiency and danger of any lamp, in place of the effectual ventilation of* MODERATELY EXTENDED WORKS, especially while conducted on the post and stall, or pillar and board system (as about Newcastle), instead of the incomparably better plan, of "the long-way of working," wherein scarcely any pillars are wasted, or empty spaces left, for air or water to accumulate in!.

I lately inspected an underground Work, carried on for five years past, in the midst of a considerable Coal district, wherein the post and stall plan had entirely been followed, from time immemorial, and in which Work, its very considerable money produce *per acre*, from the same seams, had been nearly doubled by the change, to the Derbyshire or long-way of working!; and whereby, security also from accidents, is in a greater degree obtained. Here, as everywhere else, where I have had opportunities in the last autumn, of consulting experienced Coal-viewers, Agents and Overlookers, to the number of 20 or more, I have found them unanimous, in esteeming Safety-lamps no other than dangerous *toys;* and calculated to bolster up and prolong a bad system of working. While thus supported, and by the conscious honesty of my own motives, I shall not be dismayed, or remain silent, in consequence of all the expressions of "indignation," which Lecturers, Chemists, Amateurs or Theorists can hurl at me.

When Mr. M., writing in London, boasts of "the unanimous suffrages of *a general meeting of the coal-trade*," I beg to hint to him, that the Public, whom he addresses, are no more likely

* Dr. Clanny (see Phil. Mag. xlvi. p. 408), and Mr. Holmes (in p. 168 of his work on Coal-mines) seem to admit the necesssity of what I propose; but are too deeply involved in the Lamp contest, to yield me any effectual support herein.

I think

I think to rest satisfied, with the suffrages to which he alludes, than they would, in those of the wharf-keepers, constituting the London "Coal-trade." Unless those who so clamorously advocate Safety-lamps, can show, that they *are wanted* and have been *practically received*, in other coal districts (I don't mean, of course, *the Owners of particular works ordering such lamps to be provided and used*, merely because other Owners may have done so), besides the vicinity of Newcastle; the public will sooner or later, I think, see cause for, and *will inquire*, into the peculiar circumstances* affecting such Newcastle works: but having far exceeded the limits I intended, I will conclude, and remain

Yours, &c.

37, Howland-street, Fitzroy-square, Jan. 7, 1817. JOHN FAREY, Sen.

XIII. *On the Lamp for lighting Coal-mines, proposed by* J. MURRAY, *M.D. F.R.S. E.*

To Mr. Tilloch.

SIR,—IN your Journal for December I observe a letter by Mr. J. Murray, at the close of which is a reference to the lamp which I have proposed for coal-mines, which seems to call for some notice from me. From what he states of a parade being unnecessarily made with regard to this lamp, and of its being brought forward as a rival to another, he seems not to be aware that the paper describing it, which appeared lately in Dr. Thomson's Journal, is merely the original one read before the Royal Society of Edinburgh, so far back as November 1815, and which has not appeared earlier, as the volume of the Transactions containing it has only been lately published; and I thought it unnecessary to deviate from the established practice, and publish it in any unusual mode. Having announced at first the general plan, and having sent a copy of the original paper to Newcastle, as soon as possible after it was read, I conceived myself to have done all that for the sake of humanity I was called on to do; and I was not disposed to excite any party in its support, to make

* In p. 34 of the Monthly Magazine, vol. xli. I have hinted, that the inhabitants of London and the south-east parts of England, are deeply interested in such inquiry; and I would here add, particularly, as to whether the inordinate extension of subterranean works from some drawing Pits, within fifteen years past, has not been occasioned, in part at least, by an absurd act of parliament of about that standing, which gave to a few particular Pits *by name*, on the certificate of an Officer, that coals sent to London, &c. were *drawn out of such pit's top* (wherein *various seams of different values* existed below), the privilege of being mixed and sold for *the best price* of the day!!; and others for the second best, &c.—how far has this *bounty* for extending the workings underground operated?

any

any parade, or institute any rivalship with regard to it, though I considered, and still consider, the plan in all its extent as preferable to any that has yet been brought forward.

I should not, however, have thought it necessary on account of these circumstances to have taken any notice of this gentleman's letter, did I not consider myself called on at the same time to reply to the claim which he has lately advanced, indirectly by the medium of Sir H. Davy and Mr. Hodgson (Phil. Mag. vol. xlviii. p. 352), and now directly to the invention of my lamp. This he had before brought forward in a provincial newspaper; and the best reply I can make, I conceive, to be that which I addressed at the time to the editor of a newspaper here, in which his letter had been transcribed—a copy of which, as follows, I request you will do me the justice of inserting.

"To the Editor of the Edinburgh Star.—Sir, a letter having appeared in your paper of Friday last, in which a claim is brought forward for the invention of a method of lighting mines, so as to guard against the explosion from the kindling of fire-damp, similar, it is said, to that which I lately proposed, and this being accompanied with insinuations that the latter has been derived from the former, which it seems was announced in a book published some months ago by the author of the letter alluded to—I think it necessary, from this latter circumstance particularly, to notice what I should otherwise have passed without observation, and to say that I have never seen this book to the present moment; nor, on inquiry, have I been able to learn that any person in Edinburgh has seen it, and that I never heard the most distant notice or hint of the plan mentioned in it, until I read the letter in the newspaper. I further assent most willingly to the demand of its author, of '*suum cuique*,' and am anxious that he should have exclusively all the credit that can be derived from his plans. He proposes '*a lantern made air-tight, to be fed through a flexible tube by air exterior to that of the mine, with a division in this pipe, or another parallel to it, to promote a proper current to supply the flame, and carry off in circulation the heated air impregnated with carbonic acid gas.*' Any one having the slightest knowledge of mines, or acquainted with the principles on which a current of air is established, will at once perceive that this is impracticable and absurd. He proposes also '*an air-tight lamp, having a central tube entering into a vessel of lime-water, which should be frequently agitated to extract the carbonic acid gas formed,*' a proposal which exceeds, if possible, the other in absurdity: though even this falls short of a third suggestion, which the author conceives to be of vast moment, that '*of a double recurved tube affixed to the escape-pipe, having the lower bend interrupted, and the two ends passing into lime-water,*

water, to prevent the escape of the carbonic acid into the mine.' I regret that this gentleman should have put me under the necessity of making these observations, and should have been better pleased had he satisfied himself with stating his own plans.

"I am, sir, yours, &c.

"Edinburgh, Dec. 18, 1815. J. MURRAY."

Subsequent to this I was informed that I had been attacked with much violence by this gentleman in some provincial newspapers. These attacks I had no opportunity of seeing, nor did I make any inquiry with regard to them; and I only notice the circumstance at present to say, that should any thing of this kind appear in reply to this letter, I shall not consider it necessary to pay any attention to it.

I remain, sir,

Your most obedient servant,

Edinburgh, Jan. 18, 1817. J. MURRAY.

XIV. *On the Safe-lamp, and the Ventilation of Mines. By Mr.* J. MURRAY.

To Mr. Tilloch.

SIR, — IN virtue of my promise I shall now advert, and that but briefly, to the methods I have proposed to the Society for Prevention of Accidents in Coal Mines, to aid the purposes of ventilation. And here let me premise that I firmly believe the present system of ventilation to be *radically defective*:—so far as it goes, it proceeds upon the well-known principle that aërial fluids will flow towards the source of heat and rarefied medium: but it ought to conjoin with this, a provision for the *specific gravities* of those gases which the miner has to contend with; namely, hydrocarbonate and carbonic acid gas.

Towards this desideratum approximations have been made by the ingenuity of Mr. Ryan and Mr. Menzies. It may be questioned, however, whether these ingenious plans can in every case be made practically effective:—repeated and extensive dislocations would likely render an insulation of the coal-field extremely difficult, if at all to be accomplished; and there are other reasons which will readily present themselves to the reflecting mind,—as the difficulty of determining the limits of the strata, and the like. Add to this, elastic volumes cannot rest upon each other, without mingling together; the liberated hydrocarbonate would in the very act of ascent diffuse itself sufficiently through the air of the mine to complete an explosive mixture.

The

The Safe-lamp.

An inspection of fig. 1. will explain the structure of the safe-lamp proposed by me. A is a cylindrical glass envelope, or one of horn secured from accidental external injury by being surrounded with wires; *a* is the flexible hose which feeds the lamp, by air contiguous to the floor of the mine. I have already contended for the priority of suggesting the structure of a safety-lamp founded on the specific levity of the fire-damp, and a comparison of dates will confirm to me the award. *b* is a hollow metallic ball attached to the chimney. This spherical bulb will be kept constantly supplied with carbonic acid gas and azote, the products of combustion; and therefore should a dislodgement of fire-damp from the floor of the mine take place, and the included flame expand upwards, it would never come in contact with the external atmosphere above, for it would cease to be flame so soon as it encountered the mephitic airs and aqueous vapour included in the ball of the chimney. As for explosion, this would be prevented by the heated rarefied medium within the cylinder of the lamp. Experiment taught me also (and I was led to it, by considering that a fowling-piece not correctly and evenly bored would inevitably burst) that explosive mixtures of gases would not pass through a *spiral tube;* and such a metallic canal, I also proposed for a chimney.

The Choke-damp.

For the removal of the carbonic acid gas I suggested the propriety of sinking a well in the lowest part of the pavement of the mine, as its recipient, and inclined grooves or gutters terminating here,to conduct the gas to this reservoir, from all parts of the field of coal. As carbonic acid gas may be pumped out like liquid matter, the appendage of an air-pump to the steam-engine, with a hose connecting the pump-tree with the cavity in question, would as effectually remove it as if it were so much water; this I have made *repeatedly* the subject of experiment:—or, in lieu of this, a cistern partly filled with lime-water in the well in question would, by being agitated at intervals (by vertical machinery like that of a common barrel churn), be made to condense and absorb an estensive volume of the gas, and the chalk so formed would be precipitated. A fresh portion of quicklime being thrown into the cistern and agitated therein, would yield lime-water, and be made to absorb a further volume, and so on.

The Fire-damp.

An examination of fig. 2. will convey an idea of the proposed apparatus.

apparatus. A is a common *air-tight* stove*; *a*, a flexible hose bringing the fire-damp from its lodgement to the source of heat; *b b* two metallic canals, one on either side, that opposite to the hose supplying atmospheric air as a pabulum for the combustion. This air is not suffered to mingle with the fire-damp below the bars of the grate, consequently explosion will not take place; and it will not burn at the orifice of the metallic canal, there being no oxygenous medium to support it; *cc* are two dampers which may be shut when the door of the stove is opened to admit a fresh supply of fuel; *d* is an air-tight drawer to receive the ashes.—It may be unnecessary to be more explanatory.

I am, with high respect, sir,

Your obliged and very humble servant,

London, Jan. 21, 1817. J. Murray.

Fig 2.

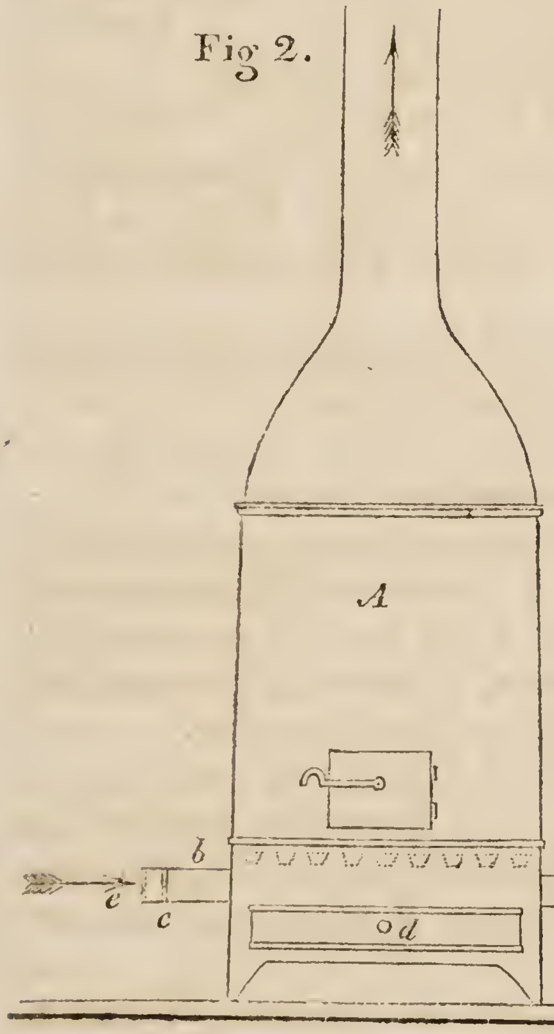
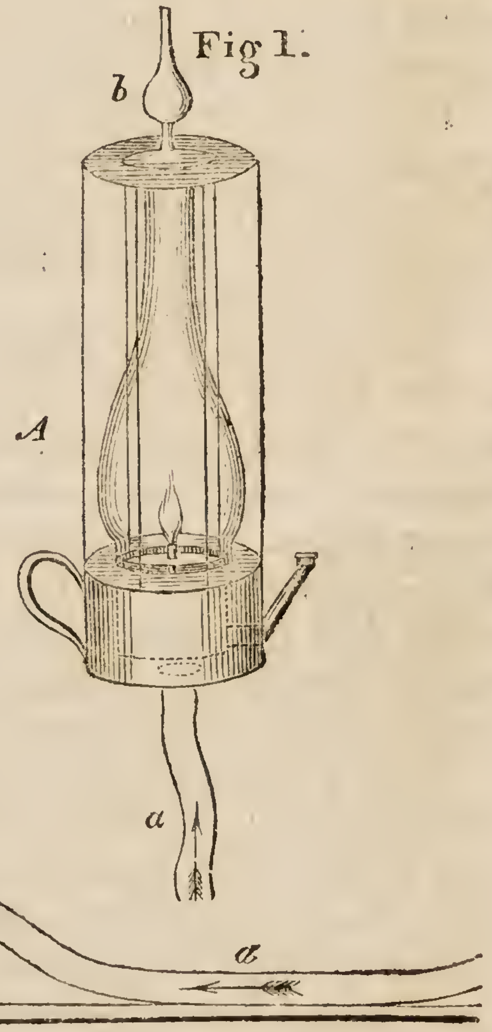


Fig 1.



XV. *On the Order of the Upper Strata of the South-east Part of England. Deduced from a Series of Observations made for Sir* Henry Englefield, *in the Years* 1811,-12,*and*-13, *by* Thomas Webster, *Esq. M. G. S*†.

Alluvium. The ruins or detritus of regular strata, formed either by the present existing causes, or by some extraordinary

* Placed under the *upcast shaft*.

† From Sir Henry C. Englefield's superb work, a "Description of the principal picturesque Beauties, Antiquities, and Geological Phænomena of the Isle of Wight." 1816.

and unknown agents. It is composed chiefly of water-worn fragments of flints, mixed with sand and clay in various proportions.

Upper Freshwater Formation.—This (in the Isle of Wight) consists of a calcareous rock, in which numerous fossil fresh-water shells are imbedded. It agrees in character and situation with the corresponding formation in the basin of Paris, and other parts of the continent of Europe. Traces of a freshwater formation are to be observed also in the London basin, between the alluvium and London clay, consisting of marl with fresh-water shells, and containing also numerous bones of land animals, as the elephant, hippopotamus, buffalo, elk, ox, &c. These have been found chiefly at Sheppey, Brentford, Essex, Suffolk, and Norfolk. In other places, as at Sheppey, Emsworth in Sussex, &c. vast quantities of the fruits of tropical countries have been found in a corresponding situation.

Upper Marine Formation.—This bed consists of blueish or greenish marl and clay containing a great number of fossil marine shells, which in general are different from those found in the London clay. It is known in this country with certainty only in the Isle of Wight.

Lower Freshwater Formation.—This formation is ascertained in the Isle of Wight. It is placed under the last, and consists of clay, marl, and sand, with vegetable matter resembling an imperfect coal or peat, and contains numerous fragments of fresh-water shells. At the bottom is found a mixture of marine with fresh-water shells. As the alternation of marine with fresh-water strata has not been observed in any other part of this country except the Isle of Wight, the traces of a fresh-water formation in the London basin cannot perhaps be referred to this.

Sand without Shells.—In the Isle of Wight this sand is extremely pure; it is dug at Alum Bay, and is used for making the best glass. The Bagshot sand perhaps belongs to this, and possibly the Grayweathers; but the positions of these have not yet been accurately determined.

London Clay.—This is the blue clay of London, Highgate, Brentford, Sheppey, Portsmouth, Stubbington, Hordwell, Southend, Harwich, &c. It is distinguished by its septaria, and its beautiful and numerous organic remains. In Alum Bay it is the most northerly of the vertical strata. Bognor Rocks are subordinate to this bed. It agrees in its fossils and geognostic situation with the lower beds of the *calcaire grossière* of the Paris basin.

Plastic Clay and Sand.—The clay in this formation is often extremely pure, and fit for the potter. It is much employed in the potteries in Staffordshire. It is seen in Alum Bay, the

trough

trough of Poole, and at the bottom of the blue clay in many parts of the London basin. An imperfect coal, or lignite, also frequently occurs in it. This formation corresponds to the French plastic clay, which lies over their chalk.

Chalk with Flints.—This formation in England extends from Flamborough-head in Yorkshire to a little beyond Lyme Regis in Devonshire [Dorsetshire]; and, where it is not covered by the beds above, forms chalk-hills or downs. It is distinguished by the regular layers of flint nodules.

Chalk without Flints.—The inferior bed of chalk in the south-east part of England is always without flints. When the chalk with flints is wanting, it forms the surface. The relations of both may be seen at the Culver and Compton Bay in the Isle of Wight, Handfast-point, Beachyhead, Guildford, Dorking, &c. It differs from the former only in the absence of flints, in the beds being thicker, and the chalk being sometimes a little harder.

Chalk Marl.—This bed consists of chalk and an intimate mixture of clay. It is always found below the two last strata. It may be readily distinguished from chalk, by its falling to pieces on being wetted and dried again. Some varieties of it, when burnt, form an excellent cement for building. It is also a valuable manure.

Green Sand-stone.—The formation to which I have given this name, consists of siliceous sand united by calcareous matter, and contains also mica and green earth. From the variety in the proportion of the latter ingredient, it is by some divided into the green sand and gray sand; a distinction which cannot always be made, since these alternate, and pass into each other. It is found in the wealds of Kent and Sussex, at the foot of the chalk downs, and is dug at Rygate and Mearsham for firestone. It is seen also at Folkstone, Beachyhead, the Culver and Compton Bay in the Isle of White, Pewsey in Wiltshire, &c. Alternating with it are often beds of limestone, as at Maidstone in Kent, where they are called Kentish rag; also in the Undercliff, Isle of Wight, beds of chert occur in it. It abounds in organic remains.

Blue Marl.—This bed may be seen under the former very distinctly in the Isle of Wight; as at Sandown Bay, many parts of the Undercliff, Niton, and Compton. It contains very few fossils.

Ferruginous Sand.—This denomination is given also to an alternating series of siliceous sandstone, clay, and limestone: the sandstone contains always more or less oxide of iron, sometimes in such quantity, as in the wealds of Kent and Sussex, that it was formerly [and still occasionally at Ashburnham] employed as

 an

an iron ore. The clay tracts of the Wealds belong to it. This formation may be also seen at Sandown Bay, Blackgang, and Compton chines, Swanwich-bay, Hastings, Tonbridge Wells, &c. Fossil shells are rarely found in it, but carbonized wood is met with in abundance.

Purbeck Shell Limestone.—This formation consists of numerous beds of shells and fragments of shells cemented together by calcareous spar, and alternating with shells and marl. The Purbeck, and perhaps the Petworth marbles, form part of this series: and it is further remarkable for containing numerous fresh-water shells, and bones of the turtle: hence it is not improbable that part of it may have been formed in fresh water.

Clay with Gypsum.—At Swanwich in Dorsetshire this is dug under the shell limestone. The gypsum does not occur in great quantity, but is employed for plaster.

Portland Oolite. This includes the stone of Tillywhino and Windspit quarries, called the Purbeck Portland, and that from Portland island. It is entirely calcareous, and is formed of small grains or concretions adhering together. It is the only stone used for the fronts of public buildings in London. Some of its beds contain many marine fossils, also fossil wood and chert.

Bituminous Shale containing the Kimmeridge Coal.—This maybe seen at Kimmeridge, Encombe, and the Isle of Portland. It is the lowest stratum visible in that part of the country to which the above observations have extended.

XVI. *Notices respecting New Books.*

MR. MURRAY's Elements of Chemical Science, second edition, with additions, is in the press, and will be forthwith published by Messrs. Underwood of Fleet-street.

This edition will contain a succinct and lucid view of those discoveries which have of late distinguished the rapid and brilliant march of chemical science.

The article on *safety-lamps* for mines, and account of experiments made by the new blow-pipe with a condensed mixture of oxygen and hydrogen, will possess considerable interest.

Dr. Spurzheim is engaged in publishing a full reply to the various reviews and other writers who have opposed his peculiar doctrine of the brain.—His large work on Insanity is nearly ready for the press.

Mr. Copland Hutchison, late Surgeon to the Royal Naval Hospital at Deal, &c. has in the press "Some further Observations on the Subject of the proper Period for amputating in Gun-shot Wounds; accompanied by the official Reports of the Surgeons employed in His Majesty's Ships and Vessels at the late Battle before Algiers."

XVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

After the vacation for the holidays, the Society met on the 9th of January, and at this and a subsequent meeting on the 16th it was occupied with the reading of New Researches upon Flame, by Sir Humphry Davy, which are of a most singular and interesting nature.

Sir H. Davy divided his New Researches upon Flame into four parts. In the first he considered the effects of rarefaction by the air-pump, and of condensation on flame. In the second he examined the phænomena of rarefaction by heat, in relation to flame and combustion. In the third, the effects of the mixture of gaseous substances not concerned in combustion or flame. And in the fourth he described various practical and theoretical applications of the results of his researches. Rarefaction by removing pressure he finds, contrary to the opinion of Grothers, has no influence on combustion, except in relation to the heat produced in rarefied atmospheres. Combustion continues in rare atmospheres as long as sufficient heat is produced in the process to effect the combination of oxygen with the inflammable basis: and the degree of rarefaction at which flames are extinguished is different for every species of flame:—those that require little heat for their combustion burn in much rarer atmospheres, and likewise those that produce much heat in their combustion. Carburetted hydrogen, which requires a strong heat for its combustion, and which produces comparatively little heat, is extinguished in an atmosphere rarefied only four times; whereas sulphur, which requires very little heat for its combustion, burns in an atmosphere rarefied 20 times; and phosphorus, which requires only a common temperature for its combustion, burns in an atmosphere rarefied 60 times. By heating atmospheres or explosive mixtures which have ceased to support combustion, or to explode *from rarefaction*, combustion or explosion may be made to take place. Sir H. Davy's general conclusion in the first section is, that, whether the atmosphere be rarefied or condensed, combustion takes place at the same temperature in it; and that the diminution or increase of heat, from the smaller or larger quantities burning in rare and condensed atmospheres, are the real causes of the extinction, diminution, or increase of combustions in them.

In the second section Sir H. shows, in direct opposition to Grothers, that expansion by heat uniformly increases the combustibility of gaseous mixtures. He states a fact particularly curious and interesting from the application he has made of it in

 another

another paper; namely, that there are slow combustions without flame, and which take place at temperatures below the point of the visible ignition of metal. He determines in this part of the paper, that the electrical spark or flame does not produce heat by compression, but by expansion by an actual increase of temperature.

In the third section he shows that different elastic fluids have different effects in extinguishing flame; that nitrous oxide is the lowest, olefiant gas the highest, as to this power: and this does not depend upon capacity for heat or density; but on an actual power of abstracting heat, which is much highest in the combustible gases, and which seems analogous to *conducting power* in solids and fluids. Steam has very low powers of preventing explosion, and azote has low powers compared to inflammable gases. The increased cooling power of the azote in condensed mixtures prevents the combustion from increasing very rapidly, and the diminished cooling power in rarefied atmospheres interferes with a rapid diminution of the heat of combustion; so that at all pressures which can occur at the surface of the earth, the atmosphere still retains the same relations to combustion.

Sir Humphry began the fourth section by illustrating the simple explanation which he has always given of the operation of wire gauze in intercepting flame, by some decisive experiments. He pointed out the limits of the size of the wire and the mesh, which differ for different flames;—that of phosphuretted hydrogen and sulphur requiring the smallest mesh, and that of carburetted hydrogen or fire damp being stopped comparatively by the largest mesh; and the law being always as the *heat* required for burning the gas, and as the heat produced by the combustion of the gas. To prevent explosion from passing from a mixture of oxygen and hydrogen requires 7000 or 8000 apertures to the square inch, whilst carburetted hydrogen and atmospheric air will not pass their explosion through a mesh containing 700 or 800. He refers to the form of the miners wire-gauze lamp, which he has adopted after many experiments, and which under all circumstances has been found to offer absolute security. By the increase of the combustibility of gaseous mixtures by *heat*, he explained the effects of various lamps and fire-places.

He stated, in speaking of meteoric appearances, that falling stars could not be owing to the combustion of *gaseous* meteors; but that they must be *solid* ignited masses moving with great velocity in the upper regions of the atmosphere.—It would exceed our limits to attempt to enter into all the applications of these researches; and indeed such an attempt would be incompatible with the nature of the investigations, many of which were too

profound

profound to be understood by merely hearing the paper read: but they offer throughout new views of the doctrine of combustion and flame, and practical applications of these views.

On Thursday, Jan. 23, another paper was read by Sir H. Davy, in which he describes some experiments for rendering invisible combustions visible. By producing these combustions by a heated wire of platinum or palladium, the wire becomes ignited, and continues so as long as the combustion can go on. A simple mode of illustrating this new principle is by gently heating a fine wire of platinum having its extremity coiled three or four times, and plunging it into a wine-glass containing a few drops of ether; it will become *red hot*, and continue red hot as long as any vapour of ether mixed with air exists in the glass. It is a combustion *invisible*, except when it is produced by bodies that are tolerably good conductors, but of small capacity and small radiating powers.

He applies this new principle to a safety-lamp which will give light in atmospheres containing so much coal gas as not to be explosive. The *twilled* wire-gauze lamp gives a safe light in all atmospheres that are explosive; and by a little apparatus placed in the top of it, it gives a beautiful light without flame in all atmospheres, non-explosive from fire-damp, that are respirable; and is relighted into *flame* by explosive mixtures or atmospherical air.

Part of a short paper by Dr. Brewster was also read, detailing some further experiments on the effects of heat in changing the powers of bodies in polarizing light. The Society then adjourned till the 7th of Febrcary.

CALCUTTA ASIATIC SOCIETY.

At a late meeting of this Society several very interesting communications were submitted to its consideration. Amongst these was the journal of Mr. Fraser's Tour to the Sources of the Satlej and Jumna, and thence across a most difficult and interesting country to the sources of the Ganges. A long and curious document was also communicated respecting several classes of robbers and murderers, known in the south of India by the name of Phansesgars, and in the upper provinces by the appellation of Thugs: they live in a regular society, and roam the country in gangs under a regular Sirdar or chief. The communication was sent by Dr. Sherwood, from Madras, and was illustrated by several extracts from official reports made in this part of India. An account of the sea-snakes that made their appearance some time since in such numbers at Madras was also forwarded by D. M'Kenzie: these snakes prove to be venomous in a very high degree; but the establis ment of medical aid near the spot, and the ready application of the eau-de-luce, has prevented

vented any great loss of lives.—Two short papers were also read: One on the ceremonies observed at the coronation of the Colastri Rajah on the Malabar coast, by Mr. Brown; and another, on several ancient coins struck by the Parthian kings, about 250 years before Christ, and which were presented by Dr. Robinson to the Society.

XVIII. *Intelligence and Miscellaneous Articles.*

POTASSIUM.

By an improved process, this substance may now be obtained in great quantities, and exceedingly pure. A few days ago an experiment was made by a French gentleman, in the laboratory of the Royal Institution, by which about an ounce of potassium was obtained from three or four ounces of potash. The apparatus employed was a double-recurved gun-barrel, similar to that usually employed; but connected at one end with a tube of glass descending into an open vessel of mercury, which by the pressure of the atmosphere tends to maintain a pressure within, equal to that of the atmosphere.

STEAM ENGINES IN CORNWALL.

Messrs. Lean, in their monthly report of work done by steam engines, state, that several counters of engines were idle in December.

The average work of twenty-five reported was 22,319,663 pounds of water lifted one foot high with each bushel of coals. Besides these, the following appear in the report for December.

Woolf's engine at Wheal Abraham, loaded 15·1 per square inch in engine cylinder, lifted 43,383,351 pounds with each bushel. His other engine, at the same mine, loaded 3·1 per inch, lifted 25,675,547 pounds:—his engine at Wheal Vor, loaded 12·1 per inch, lifted 35,367,233 pounds: and his engine at Wheal Unity, loaded 13·8 per square inch, lifted 32,044,976 pounds one foot high with each bushel.

The altered engine at Wheal Chance, loaded 13·4 per inch, lifted 47,048,844 pounds with each bushel.

Some of our correspondents have asked us why the lightest-loaded engine does the least work with the coals? They will understand this by asking themselves another question: Suppose the load only one ounce, what would the engine then lift with each bushel? A certain portion of the coals is consumed in merely keeping the engine in motion—whether it carries the full load or not.

DE-

DESTRUCTION OF THE EXTENSIVE COLLIERIES AT CHIRK IN DENBIGHSHIRE.

On Saturday evening, the 28th of December, owing, as it is supposed, to inattention in the servants of the Ellesmere Canal Company, the stop-gates, plugs, &c. for regulating the quantity of water on that part of the canal which is embanked up to Chirk aqueduct, were neglected: the fatal consequence was, that the embankment being overpowered by the great weight of superfluous water, gave way, and falling down a precipice, completely dammed up the river Ceriog, which flows below it, and over which the canal is continued by an aqueduct. The water being thus impeded, quickly found its way in another direction, and in half an hour every pit belonging to the colliery was filled with water, earth, gravel, &c. The machinery was torn in pieces by the tremendous force of the current, and very considerable damage done to the surrounding country. Most providentially this was the only night in which, for several years past, the workmen were absent from the pits. They had been allowed a little time to collect Christmas bounties, &c. and thus this dreadful calamity is not aggravated by the loss of the many valuable lives which must have been otherwise inevitably sacrificed. All the horses employed in the works were drowned. The immediate loss to the proprietors of the works is immense; and the destruction of so valuable a colliery, which has for a long series of years, produced fuel for the country, and employment for its poor, is a public calamity.

EXPEDITIONS TO AFRICA.

The particular details of the unfortunate expedition to explore the river Congo have since our last publication reached the Admiralty. It appears that the ship Congo under the command of captain Tuckey, with the Dorothy transport in company, arrived at the mouth of the Congo about the beginning of July. The Congo having been purposely built to draw little water, it left the transport, and proceeded alone up the river, to the extent of 120 miles, when her progress was stopped by rapids and other difficulties of the current. Captain Tuckey with all the principal individuals attached to the expedition then disembarked, resolving to prosecute their survey by land. The country as they proceeded became barren and mountainous; and the climate, though mild (Fahrenheit's thermometer seldom exceeding 76 degrees by day, and never descending below 60 at night,) was so excessively dry that it became extremely difficult to procure water. The people are represented as timid and pusillanimous: they opposed no obstacle to the progress of our adventurers, and readily furnished whatever guides were required.

After penetrating about 150 miles, (being 120 further than any white person had ever been before,) Captain Tuckey and his com

panions

panions became at last so exhausted by fatigue and the numerous privations, especially of water, to whirh they were exposed, that they were obliged to retrace their steps.—With great difficulty they regained the banks of the Congo: but scarce had they embarked, when the effects of their journey began to diplay themselves in the most fatal form. All of them were seized with fever, and in a short time all but one (of those engaged on shore) fell victims to its fury. The following are the names of some of the principal sufferers:

Captain Tuckey, commander of the expedition; Lieutenant Hawkey, lieutenant of the Congo; Mr. Professor Smith, botanist; Mr. Tudor, comparative anatomist; Mr. Cranch, collector of objects of natural history; Mr. Galwey, a friend of Captain Tuckey, who volunteered from pure love of science; and Mr. Eyre, the purser.

In a letter from Mr. Mackerrow, the surgeon of the Congo, and who remained on board during the land expedition, the following particulars are added:

"Of the eighteen who died in the river, fourteen had been on shore, marching for some time, and were far advanced before reaching the ship.

"Professor Smith, who saw many of them when taken ill, gave to some a dose of calomel, but to others nothing had been administered.

"The fever appeared in some degree contagious, as all the attendants upon the sick were attacked, and before we left the river it pervaded nearly the whole crew, also some of the transport's; but as for myself, although constantly among them, I did not feel the slightest indisposition until we left the coast, when I was attacked: however, I considered mental anxiety and disturbed rest as the sole causes.

"Captain Tuckey had been afflicted many years with chronic hepatitis; and on returning from travelling *five weeks* on shore, he was so excessively reduced, that all attempts to restore the energy of his system proved ineffectual.

"Mr. Tudor was in the last stage of fever before I saw him, as were Messrs. Cranch and Galwey.

"Professor Smith died in two days after he came under my care, during which time he refused every thing, whether as nutriment or medicine.

"Lieutenant Hawkey was taken ill after leaving the river, and died on the fourth day; his case was rather singular; symptoms were irritability of stomach, with extreme languor and debility, but he had neither pain nor fever.

"Mr. Eyre had a violent fever, and on the third day breathed his last: before death a yellow suffusion had taken place, with vomiting of matter like coffee ground."

The

The Journals of Captain Tuckey and the gentlemen in the scientific departments are, it is said, highly interesting and satisfactory, as far as they go; and will no doubt soon be laid before the public.

LIST OF PATENTS FOR NEW INVENTIONS.

To Richard Wright of Bishopsgate Within, London, for certain improvements in the construction and propelling ships and other vessels.—6 months, 10th December, 1816.

To William Dean of Manchester, calico-glazer, for certain improved machinery for waxing calico or any other cloth or fabric previous to the process of glazing.—2 months, 14th Dec.

To Samuel Brown of Mark-Lane, and Philip Thomas of Liverpool, manufacturers of iron cables, for their invented chain or chains manufactured in a particular manner by a new process or processes, and certain apparatus and improvements in performing and executing the same.—2 months, 19th December.

To John Raffield of Edward-street, Portman-square, architect, for certain improvements on and additions to his former patent, for an apparatus to be attached to fire-stoves of all descriptions for rooms, for the removal of cinders and ashes, and for the better prevention of dust arising therefrom, which said additions may be used jointly or separately.—6 months, 10th January, 1817.

To William Manton of South-street, Grosvenor-square, coachmaker, for certain improvements in the application of springs to wheel-carriages, which he conceives will be of great public utility.—6 months, 20th January.

Dr. Merriman and Dr. Ley will recommence their Lectures on the Theory and Practice of Midwifery, and the Diseases of Women and Children, at the Middlesex Hospital, on Monday February 17, at half past ten.

Astronomical Phænomena, February 1817.

D. H. M.		D. H. M.	
1. 0.41	☽ γ ♋	12. 0. 9	☽ σ ♐
2. 0. 0	♂ 26 ♐ * 18′ S.	14. 0. 0	♀ 20 Mayer * 19′ S.
2.11. 1	☽ η ♌ a near appulse	16. 0. 0	♀ 26 Mayer * 12′ N.
4. 0. 0	☽ perigee	17. 0. 0	♀ 29 Mayer * 10′ S.
4. 5.16	☽ ν ♍	18.10.38	☉ enters ♓
5. 8.48	☽ γ ♍	18. 0. 0	♀ ε ♓ * 16′ N.
8.15.41 I }	of κ ♎ * 7′ S. of	19.17.19	☾ ♀
8.16.51 E }	☽'s centre	20 11. 2	☾ ο ♓
9. 0. 0	♀ 44 ♓ * 46′ S.	20. 0. 0	☾ apogee
10. 0. 0	☿ 385 Mayer * 30′ S.	21. 0. 0	☿ stationary
10. 0. 0	♄ 2 ♒ * 47′ S.	25. 6.29	☽ 125 ♉
10.10.32	☽ θ oph.	25.10.30	☽ 132 ♉
11.13.14	☽ λ ♐	27.10.30	☽ κ ♊ a near appulse
11.20.10	☽ φ ♐	28. 0. 0	♂ 813 Mayer * 4′ S.

METEOROLOGICAL TABLE

Extracted from the Register kept at Kinfauns Castle, N. Britain. Lat. 56° 23′ 30″.—Above the level of the Sea 129 feet.

1816.	Morning 8 o'clock. *Mean height of* Barom.	Morning 8 o'clock. *Mean height of* Ther.	Evening, 10 o'clock. *Mean height of* Barom.	Evening, 10 o'clock. *Mean height of* Ther.	Mean Tempr. by Six's Ther.	Depth of Rain. Inch. 100	Nº of Days. Rain or Snow.	Nº of Days. Fair.
January.	29·424	34·677	29·435	34·580	35·45	2·30	13	18
February.	29·651	33·724	29·683	34·000	34·58	0·80	7	22
March.	29·642	35·838	29·655	35·612	37·35	1·60	13	18
April.	29.714	39·933	29·697	38·766	41·30	1·00	8	22
May.	29·762	47·129	29·768	45·322	48·64	2·30	10	21
June.	29·812	53·400	29·821	50·266	54·46	1·30	8	22
July.	29·560	55·710	29·550	53·330	56·29	4·25	16	15
August.	29·804	54·550	29·807	53.070	56·19	3·15	10	21
September.	29·729	50·060	29·721	48·900	51·20	2·75	16	14
October.	29·726	44·710	29·710	46·320	46·58	2·15	10	21
November.	29·615	38·466	29·622	38·333	39·26	1·65	8	22
December.	29·494	33·871	29·489	33·806	34·51	1·70	13	18
Average of the year.	29·661	43·505	29·663	42·692	44·65	24·95	132	234

ANNUAL RESULTS.

MORNING.

Barometer. *Observations.*	*Wind.*		*Thermometer.* *Wind.*	
Highest, 30th Nov.	W.	30·67	21st July, SW.	62°
Lowest, 17th Jan.	W.	28·40	13th Dec. W.	15°

EVENING.

Highest, 30th Nov.	W.	30·67	14th Sept. SW.	60°
Lowest, 12th Jan.	SW.	28·60	29th Jan. NE.	19°

Weather.	*Days.*	*Wind.*	*Times.*
Fair	234	N. and NE.	32
Rain or Snow	132	E. and SE.	105
		S. and SW.	62
	366	W. and NW.	167
			366

Extreme Cold and Heat, by Six's Thermometer.

Coldest, 13th December . . .	Wind West . . .	13°
Hottest, 24th June	Wind East . . .	72°
Mean Temperature for 1816		44° 65′

Result of Three Rain Gauges.	In. 100
No. 1. On a conical detached hill above the level of the Sea 600 feet	52·43
— 2. Centre of the Garden, 20 feet	24·95
— 3. Kinfauns Castle, 129 feet	19 61
Mean of the 3 Gauges	32·33

METEORO-

Meteorological Observations kept at Walthamstow, Essex, from December 15, 1816, *to January* 15, 1817.

[Usually between the Hours of Seven and Nine A.M.]

Date.	Therm.	Barom.	Wind.	
December				
15	43	29·82	SW.	—Windy, clear, and clouds; fine; sun and wind, and *cumulostratus;* starlight.
16	32	29·43	S.	—Hazy and white frost; very fine day; dark night.
17	39	29·33	S.	—Damp and cloudy; very rainy and hazy; star light.
18	36	29·23	S.	—Clear and clouds; *cirrostratus;* cold and showers; dark and windy. New moon.
19	37	30·11	N.	—Clear and clouds, and wind; *cirrostratus;* slight snow at 9 A. M.; great snow showers, and sun; starlight; ground whitened by snow.
20	26	30·44	N.	—Fine day; bright star light.
21	22	30·44	N.	—Hazy; very fine day; snow still on ground; star light.
22	17	30·11	N.	—Fine frosty morning, and some sun; very hazy; dark; starlight.
23	30	30·10	W.	—Foggy; showers and wind; very damp; very damp and dark.
24	46	29·71	S.	—Damp and windy *cumuli;* slight showers; windy and dark.
25	47	29·63	NW.	—Mild day; very clear morning; very fine day; star light.
26	49	29·34	S.	— Rain; showers and wind; moonlight. Moon first quarter.
27	38	29·45	S.	—Clear starlight, 7 A. M.; 9 A.M. hazy; showers; sun and wind; fine star light.
28	33	29·83	S.	—White frost and hazy; hazy day; cloudy and windy.
29	42	29·44	W.	—High wind, and clear; *stratus* low, fine day; *cirrostratus* NW.; moon and *cumuli.*
30	39	29·92	N.	—Rain and wind; stormy; very rainy day; rain continued.
31	44	29·72	S.	Foggy dark *nimbus;* slight rain; cloudy.
January				
1	42	29·51	S.	—Foggy; rain and wind; showery; wind and cloudy.
2	41	29·44	S.	—Hazy; very fine day; bright moon and starlight
3	38	29·51	SW.	—Snow in the night; ground white; great floods; showers; sun and wind; moon and *cirrocumuli.* Full moon.

Date.	Therm.	Barom.	Wind.
January			
4	47	29·22	S.—Stormy; wind and rain; wind and sun; showers; moon, stars, and *cirrus*.
5	39	29·93	W.—Hazy; very fine day; floods decreasing; great snow; wind and rain; light but cloudy; floods increasing again.
6	40	29·53	W.—Rain; showers and sun; *cirrus* NW.; very red sunset; clear night.
7	34	29·21	NW.—Clear and fine; very fine day; clear star light.
8	27	29·43	N.—Hazy; white frost all day; fine day; star-light.
9	29	39·53	N.
10	28	29·54	N.—Foggy; white frost; foggy all day; dark night.—Moon last quarter.
11	25	29·44	N.—Hazy; white frost; hazy day; remarkable sunset, and orange *cumuli*; foggy and dark.
12	32	30·11	W.—Hazy; sun through fog; great fog at 1 P.M.; but cleared at 4 P.M.; dark night.
13	32	29·71	S.—Very thick fog; fog all day; rainy evening; 11 P.M. star light.
14	38	29·54	W.NW.—Clear and windy; fine day; dark night.
15	32	29·11	S.SE.—Snow, then rain, then snow again; deep snow at 8 A.M.; showers of rain and snow; star-light; deep snow.

Average of the Year 1816, of the Meteorological Journal kept at Boston, Lincolnshire.

Therm. 50·3983—Barom. 29·9016

The greatest height of the Thermometer was June 21st and 29th, 71° at 1 P.M. The least height of Thermometer was February 9th, 3° at 8 A.M.

The greatest height of the Barometer was 30th Nov. 30·76 at 1 P. M. The lowest ditto ditto was the 15th Dec. 28·87 at 1 P. M. blowing a gale from S. W. without rain.

	Therm.	Barom.
January	40·26	29·776
February	35·61	29 973
March	44·	29·86
April	49·98	29·826
May	56·	29 955
June	59·93	30·016
July	62·45	29·77
August	61·8	30·06
September	58 43	30·0014
October	54·82	29·91
November	42·1	29·862
December	39·4	29·81

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1816.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Dec. 15		40·	28·87	Fair—a gale from SW
16		38·	29·51	Very fine—heavy rain at night
17		44·	29·35	Cloudy
18	new	42·5	29·62	Ditto—frost at night
19	1	32·	30·33	Hail and snow ditto
20	2	31·	30·65	Very fine ditto
21	3	30·	30·39	Ditto ditto
22	4	31·	30·20	Fair but cloudy ditto
23	5	39·	29·88	Rain—frost went away with rain
24	6	49·	29·60	Cloudy—heavy rain at night
25	7	42·5	29·86	Ditto fair
26	8	49·	29·33	Rain
27	9	40·	29·44	Cloudy
28	10	38·5	29·78	Heavy rain with gale from W
29	11	43·	29·62	Fine—blows hard from W
30	12	35·	30·08	Snow—rain at night
31	13	47·	29.81	Rain, till noon
1817				
Jan. 1	14	41·	29·49	Rain
2	15	43·	29·48	Cloudy—frost at night
3	full	40·	29·56	Rain
4	17	59·5	29·26	Fair—heavy rain at night
5	18	44·5	29·83	Ditto ditto
6	19	43·	29·54	Ditto—frost at night
7	20	36·	30·47	Very fine ditto
8	21	32·	30·51	Ditto ditto
9	22	34·	30·65	Ditto ditto
10	23	31·	29·58	Ditto ditto
11	24	38·	40·43	Cloudy—slight ditto
12	25	40·	30·19	Fine
13	26	38·	29·69	Rain and sleet—frost at night
14	27	39·	29·61	Very fine ditto
15	28	33·	29·05	Snow

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For January 1817.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Dec. 27	40	45	36	29·42	0	Showery
28	32	45	48	·50	0	Stormy
29	43	44	40	·70	15	Fair
30	40	40	45	·60	0	Rain
31	45	47	47	·58	0	Rain
Jan. 1	47	49	49	·40	0	Rain
2	46	48	40	·46	10	Fair
3	35	46	45	·50	0	Stormy
4	45	49	47	·10	0	Stormy
5	37	44	33	·70	0	Rain
6	35	44	36	·70	0	Stormy
7	35	42	35	30·28	14	Fair
8	24	37	33	·28	12	Fair
9	28	36	32	·45	10	Fair
10	27	28	21	·45	0	Foggy
11	26	36	32	·33	6	Cloudy
12	32	38	35	29·90	15	Fair
13	32	40	38	·60	0	Cloudy
14	32	40	32	·50	7	Fair
15	32	36	28	28·80	0	Snow
16	28	38	40	·98	0	Rain
17	42	43	42	·89	7	Showery
18	43	45	40	29·00	10	Fair
19	42	43	40	·00	0	Rain
20	43	47	42	28·80	7	Stormy
21	35	42	40	29·60	10	Fair
22	43	49	45	·72	6	Cloudy
23	46	54	52	·90	5	Cloudy
24	52	54	49	30·15	5	Cloudy
25	47	51	47	·28	6	Cloudy
26	45	47	46	·09	7	Cloudy

N.B. The Barometer's height is taken at one o'clock.

XIX. *Essays on Chemical Philosophy.* *By Mr.* M. Allen, *Lecturer.*

Essay I.

It is intended in these Essays to offer a brief outline of the laws and principles of natural science; and as some of the ideas proposed to be offered, may be found to differ from the principles and reasoning which have been advanced by others, the reader is requested not to be hasty in either receiving or rejecting what may be advanced, but to suspend his decision till the way has been cleared for their statement, and their application to chemical affinity, electricity, caloric, light, astronomy, and the immense detail of facts in Nature and in chemical or artificial operations, has been exhibited.

The bare mention of the subject precludes the necessity of argument to show its importance.—*It is the desideratum of science.* It is true that the operations and effects of the power of Nature, called attraction, have been submitted to rigid and mathematical calculation: but it is equally true, that only by a misnomer have these been denominated *the laws of its action;* for they merely point out the uniformity of those effects which the laws and principles of its action produce. The same misapplication of the words "laws and principles" is committed in applying them to those circumstances by which the operation of these laws and principles is facilitated, opposed or modified, such as pulverization, solution, gravity, &c. The subject therefore requires, as has been said, no argument to establish its importance, however desirable it might be that more adequate powers than the writer can boast had undertaken the present task. He feels however no apprehensions for the fate of his attempt, for truth is the object he aims at; and so far as he may succeed in making this manifest, he knows that, in this philosophical age, he has nothing to fear from the hostility of prejudice. All opposition, however, is not prejudice. Opposition often exerts itself beneficially in defence of truth; but prejudice resists and presumes to answer before it has examined and understood the things it had predetermined to oppose.

Without further preface, I will at once proceed to the classification or division of science, intended to be pursued, and to state the reasons for adopting the names and arrangements employed in such classification or division;—and, as the primary head, what relates to attraction—attractive agencies—and passive substances.

I. *At-*

I. *Attraction.*

Attraction, in common language, is applied both to the cause, and to the effect which that cause produces; and though philosophers now use it in a more abstract and arbitrary manner, yet they appear to have chosen it in the first instance, rather as descriptive of effects, than as expressive of any abstract idea they could form or convey of its essence. In this sense Newton considered the word; and though he adopted and applied it, from the want of a more suitable word, in the most enlarged manner, he complained of those who mistook his reasons for this, and evidently considered attraction as only the effect of another power*. The term *attraction*, as well as the term *repulsion*, can express only the mere property of a power, and is therefore an improper name for the power itself; and philosophers would render an important service to science, were they unanimously to consent to introduce for the latter some other word more defined and correct in its application, as the *vis naturæ*, or cause producing attraction and repulsion, or attractive and repulsive or other effects. The reasons for this will appear to more advantage by and by. Let it suffice at present, to state, to define and restrict the term, that the reader may understand the sense invariably meant to attach to it in the remarks which may be laid before him in these Essays. The *vis naturæ* is employed as a term to express the sole grand power or principle of Nature; and, agreeably to this view, is defined to be that "*cause which produces all the motion and union of matter.*"

This general definition is offered, from a conviction that this power is alone sufficient to explain all the phænomena of Nature and of Art. Let it not be imagined, that it is intended to express or to determine the nature or essence, or even all the properties, of such a power; or, that this power consists of one element alone; for it is much more probable that its constituents may form the basis of all matter: it is not however intended that its compound nature shall be contended for or maintained. Facts alone are the only sure foundation on which the philosopher should attempt to establish his theory. Let the phænomena of facts be stated correctly, and their number. Can it be doubted, that with due attention they will be found to arrange themselves under one general principle, regulated by the same laws, modified by circumstances?—Such a generalization of nature can alone, in my opinion, furnish a true theory. An artist copies the forms, colours, and diversified appearances of nature—

* See *Attraction*, Johnson's large Dictionary, and last page of Pemberton's View of Sir Isaac Newton's Philosophy.

the true philosopher, the servant of nature, traces the operation and endeavours to ascertain the laws of that power by which all these are produced.

In this sense then I contend, that the power which in chemistry binds and unites matter together, is the same power that carries matter from one point to another; and I therefore define it to be "*that power which produces all the motion and union of matter*." According to this view of its operations, it is not necessary to call to the aid of science a repulsive or projectile power, in order to explain either the minute or extended movements and changes, or "motion and union," of matter, which are for ever going on in every part of the creation.—Nature is but another word for this power. The operations are the objects of all natural science; and circumstances alone constitute the difference of each of our artificial divisions of human knowledge. It has been said "that electricity, caloric and light are powers by which repulsion is produced; and the proofs adduced in support of this opinion are, that, by the application of these in an increased degree, "repulsion is established between the particles of matter, and the force of attraction is overcome:" and hence, it is said, "solids become fluids, and fluids gases." But these effects, as well as every other attributed to a repulsive power, may, it is believed, be better explained without the introduction of more than one universal power of Nature.

Repulsion is a term which can never be used with propriety as expressive of a distinct cause of effects, contrary and in opposition to those of attraction, if by that word we mean the power of Nature. It may indeed be used as a descriptive term, and to this it ought to be restricted. We may speak of *certain powers having a repulsive effect on matter, but never* (*with propriety*) *of repulsion itself as the power or primary cause of any effect whatever.*

It is said, "the magnetic and electric attractions so far agree with the attraction of gravity as to operate at sensible distances, and even, as has been attempted to be proved, according to the same law;" and yet these powers with caloric and light are classed together in the same system of chemistry*, "as the powers by which repulsion is produced." In fact, this definition of repulsion is not the definition *of it as a cause*, but of certain effects *of other powers, to which the name "repulsion" is applied as descriptive of their kind or mode of operation. And in this sense alone its application is proper.* The definition says in direct words, "*Electricity, caloric, and light, are* (*themselves*) *the powers by which repulsion* (*itself*) *is produced;*" and yet to

* Dr. Murray's System, 1st, 2d, 3rd and 4th editions.

the consideration of this word, not as a descriptive term of effects, but as a name for a distinct power of causation, five pages are devoted for one that is given to attraction, as the grand and universal power of Nature, of whose effects Newton traced with an unerring hand the grand and extensive outlines.

The same cause will always produce the same effects, the circumstances being the same. Now these powers, electricity, caloric, and light, are invariably treated as powers principally concerned in the attractions which take place among the particles of matter, or chemical attraction. How then are they said to produce effects the very reverse of attraction? In fact, this confusion has originated in the arbitrary and undefined application of the term *attraction:* it is one cause which produces different effects, either (as will hereafter be proved in detail) *from difference of circumstances, or in the degree of the same power*—from these causes arises its two-fold action, of carrying bodies and the particles of bodies in one direction, and drawing them in another.

Attraction binds matter together, and the same power (I contend) carries it from one point to another. The latter is the *repulsion* of chemists, but the *repulsive effects* of Nature; and these varied modifications of the same power serve but to exhibit so many instances of the superior and all-pervading omnipotent influence of that power. Artificial and arbitrary distinctions have made Science and Nature two very different things. Chemistry has been defined as "the science which treats of the insensible motions of matter," or say "of the minute and intricate changes of Nature." Now, if the minute and intricate changes of Nature are connected with her sublime and extended movements,—if minute atoms and celestial masses are moved by the same power, regulated by the same laws, and merely modified by circumstances,—then this definition is defective, and it only applies partially. If then it be allowed that the object of chemistry is to examine the one, and that not only the most difficult, but which must contain the first principles of the other, that other and subsequent part of natural science is a continuation of the first, and they ought to be considered as parts of one whole. *Chemical philosophy** I would therefore define to be that science which investigates the movements and changes of Nature, and endeavours to ascertain the laws and principles by which these effects are produced. This is its proper object—it is the golden key by which we unlock the secrets of Nature, and are enabled to discover the laws of that power by which Nature produces all her operations.

* Definitions of its application to practical chemistry and astronomy will be given hereafter.

Distinctions

Distinctions are made between it and other branches of science, making it a separate and insulated portion of human knowledge. Whereas every student must have observed that, in every advance he makes in chemistry, his views of Nature are enlarged, and he is enabled more correctly to ascertain the laws and principles of the operation of the grand power, the cause of all the motion and union of matter. The further therefore we advance, the more these artificial divisions of science will be confounded, and the more certainly will be preindicated a time when they will be lost in the magnitude and splendour of truth.

Notwithstanding these partial definitions and arbitrary distinctions, every description we have of the powers, the objects, and the application of this science does more justice to its importance, and develops, not an insulated portion of knowledge, but, in fact, all that we include by the phrase Natural Philosophy, and, more than this, the word Chemistry implied and included. I am firmly persuaded that improved chemistry, or, which is the same thing, correct views on the laws which attraction or the power of Nature observes, will simplify science, and render its acquirement easy, delightful, and rapid. It will not appear strange that I should hold chemistry destined to this honour, after what has already been said; nor do I argue so much from the wonders it has already accomplished, as from the nature of the science. Its professed object is to remove the veil from the face of Nature,—to discover the wonderful properties of matter,—and to become acquainted with the mysterious power by which they are made to produce the various movements and changes in the order of things, as well those of Nature, as those of art in civilized society. Does it not comprehend the elements of every science? Is it not the centre and circumference of a great circle, in which they all have their origin and termination? Can it be otherwise, when its sole consideration is the *primum mobile* of matter?

[To be continued.]

XX. *On Barometric Pressure; Whether affected by being posited beneath a Balloon in the Act of ascending or descending? By* A Correspondent.

To Mr. Tilloch.

Sir, — A conversation among some friends a few evenings since occurred, respecting barometrical pressure; and whilst endeavouring to account for some of the phænomena presented by

 the

the atmosphere, a question arose respecting the forces immediately under a balloon; and more particularly, the pressure of the atmosphere downwards.

I venture to send to you a few questions, which have arisen in my own mind, during a short consideration of the subject; intended, if you think fit, for insertion in your Magazine. I refrain from giving you what I have heard on the subject, because the conversation in which it occurred was private;—and my object at present is, to make a confession of my own ignorance of this part of experimental philosophy, and request information, through the medium of your Magazine, from those who are able to give it.

If a balloon in a state fit for ascension is liberated from the earth's surface, I conceive the barometrical pressure beneath, is diminished for a short time; *i. e.* whilst the balloon remains in the vicinity of the spot from whence it was liberated: because, as the balloon, with the column of air above, is lighter than the collateral corresponding columns; it, whilst rising, must cause a diminished downward pressure on the spot beneath. It has been said to me, that, the pressure of the air taking place in all directions, that of the columns surrounding the balloon will be exerted as well under it as laterally, and produce the same effect there as on any other part. This I cannot conceive to be the case, until the balloon is so far removed from the earth's surface, as not to influence the air on it; or, in other words, until the distance is such, that of all the air which flows in to supply the place of the balloon as it rises, none comes from the earth, or from that part surrounding the barometer. But whilst the air which replaces the balloon forms a current enveloping the instrument, the mercury in it I think would descend.

I dare not positively affirm that I am right in these conjectures, and therefore ask your assistance to obtain information concerning them—but I will describe to you an experiment I made on the subject.

I bent about two inches of the end of a long open glass tube upwards to an acute angle, and then immersed it to some depth beneath the surface of water, in a large vessel; inclining the longer leg of the tube, so as to remove the point where it touched the surface of the water to some distance from the spot immediately over the lower opening. In this situation, the tube contained a column of water which could not be affected by the general mass in the vessel, except at the lower end. Then, I was able by means of the lungs, and a large tube, to liberate considerable bubbles of air in the water at any part. Permitting the bubbles to escape over the orifice of the tube, they caused a con-

a considerable descent of the column of water within it, evident by the depression of the upper and distant surface: if they were liberated beneath, they did not affect it until they had reached the orifice, and then acted as before: if the bubbles were liberated at a little distance above the orifice, the effect was not so great, and at a certain distance was not sensible.

Perhaps I may commit an error in comparing the inelastic dense medium of water, to the elastic rare one of air: however, I shall venture to do so; for I am not aware that any circumstance could take place, were the experiment repeated in the latter, which would give results differing in their nature from the above. The strength of the forces brought into action in the two *media* cannot of course be considered equal, but they appear to me to be of the same kind.

It occurred to me also, whilst making the above experiment, that the descent of a body in any medium should increase the pressure beneath it; and in water, I easily succeeded in elevating a column above its ordinary level, by letting a body descend towards the lower extremity of it. Thus, by permitting a leaden weight to sink on to the end of the tube; or, by thrusting a solid body towards it; or (as I at last made my experiments) by depressing a circular plate of metal attached to the end of a stout wire, I caused a rise in the column of water inclosed in the tube.

I have not had time further to consider the effects; and as it would be an easy task to many of your readers to solve what to me would be difficult, I venture to ask a few questions on the subject.

A barometer being placed under a balloon; does it not indicate a diminished pressure on its ascent? does not the pressure increase as the balloon recedes? and in what ratio?

A balloon or any large body descending in the atmosphere; should it not on approaching the earth increase the pressure beneath? and in what proportion?

A barometer on the earth's surface being affected by the ascent of a balloon from the spot where it is placed; how should a barometer be affected in the car of a balloon? should it not mark a diminished pressure whilst rising, and an increased pressure whilst descending, to what it would do in the same situations were no balloon present?

The above questions might be put more generally, as on the motion of bodies in *media;* and many others might be asked: but were these answered, I should be able to solve the rest myself, and I prefer putting them in this form because they refer more directly to the conversation I have before noticed. Should

 the

the supposition contained in the last question be correct, the method of estimating the height of balloons by barometers must be very uncertain.

I am, sir, yours sincerely,

Dec. 16, 1816. —— F.

XXI. *Medical Premium of Fifty Guineas offered by* A Correspondent.

To Mr. Tilloch.

Sir, — During the reign of pedantry, knowledge that was not dug out of a folio was considered as trifling, and beneath a man of letters. But now, the diffusion of science in periodical publications has been found so useful to mankind, that men of the first eminence do not disdain to send their contributions to literary journals.

I was led to these reflections by some useful medical papers, which I have lately seen in the Philosophical Magazine, and in other publications, in which medical knowledge of the first importance has been communicated to the public.

The columns of that excellent paper The Star have lately been filled with answers to a riddle, which it was said had been proposed by Miss Seward. This good-natured condescension could only be attributed to the liberal desire of giving to some of various competitors the chance of gaining 50*l.* I have reason to doubt the existence of the legacy which has given birth to so many rhimes. But I now, sir, beg leave by your means to offer *fifty guineas* reward, for the solution of a problem which would be really useful to my fellow creatures. It is a case in medicine; and I promise to pay fifty guineas to any person who shall point out a cure for the disease, or a part of that sum proportioned to the degree of palliation which may be produced by his prescription.

As I have been your correspondent from the commencement of your Magazine, in 1797, to the present time; and as I have by other means been known to you, you will, I think, trust to the performance of my promise.

Should this public application be successful, it would I think open the way to similar proposals for the advancement of the practical part of medical science, giving a motive and an opportunity for the exertion of latent merit.

I am, sir,

Your obedient servant,

Z.

Z. of

Z. of seventy-two years of age, of uniformly temperate and active habits, and of health in general good, though not robust, was afflicted twenty years ago with the jaundice arising from gallstones which have not since troubled him; and three years ago, with a *hernia humoralis*, which suppurated, and left the patient considerably debilitated. Some time after this disease, his stomach became disordered, and has ever since been liable to loss of appetite, extreme flatulency, and frequent vomiting. This vomiting has usually occurred at night, about eight or ten hours after dinner. Sometimes, but rarely, food was ejected from the stomach. In general a bitter fluid, resembling bile, was thrown up. The violence of these symptoms sometimes intermitted for three or four weeks, and then returned again, as if from some noxious accumulation in the stomach.

Of course the patient was weakened and somewhat emaciated by the continuance of this disease; but his spirits, and the activity of his mind, have at no time been depressed. Every article of diet that is considered as flatulent had been at different times sedulously excluded. Animal and vegetable food have been tried alternately and in conjunction. Abstinence from wine, and from all fermented liquors, has also been tried; and these trials have been made with care by a person used to experiment, and whose appetite is entirely subservient to control; and from repeated trials, he has found nothing which has appeared peculiarly obnoxious to his stomach, but fried fat, and the skinny or sinewy parts of meat, and the acetous acid.

At different times, wine of all sorts, and malt liquor, have been disagreeable to him. At other times, Port wine, mixed with twice the quantity of water, has been his beverage at dinner, with sometimes one, or at most two small glasses of Port wine after dinner. But malt liquor, viz. small beer of a *good* quality, has seemed to agree with him better than any other liquid. Water, which in his neighbourhood is excellent, has not been found salutary—for whether water, beer, or wine, or meat, or vegetables, or simple puddings, or roasted apples, made part of his diet, the same inordinate flatulency and sometimes subsequent vomiting have recurred. It is to be observed, the acidity which usually attends common indigestion has not been a symptom in this disease.

The bowels have in general been kept in a proper state by a small quantity of aloes taken in the form of Darwin's dinner pills, which has never produced any constriction nor any excess.

Various remedies have been proposed by the medical friends of the patient, who is happily known to many men of medical eminence. Opium, bitters, alkalies have been tried; but every kind of drug seems to have been ineffectual. Indeed the patient, though

though he has the most sincere regard for individuals of the medical profession, and a high opinion of their skill, is not at all inclined to tamper with his constitution.

There is another symptom attendant upon this disease, which must be mentioned. Ever since his recovery from the *hernia humoralis*, three years ago, his urine has never returned to its natural colour, but has constantly been of a wheyish appearance, yet without flocculi or gluten. It has been voided in usual quantities in the day time, but with unusual frequency in the night. At times the urinary secretion has been affected by a strange propulsion of wind from the urethra, attended with a considerable hissing or explosive noise, and ejecting a white fluid that has the exact appearance of *cuckoo-spittle;* but which, on being touched, appears to be nothing but common urine mixed with air, without any gelatinous consistence. This symptom is always attended with a slight distension of the rectum, so that the patient cannot determine the source from whence this gas is evolved; but this affection is not attended with any pain or inconvenience.

The pulse has usually been regular, beating from 72 to 86 in a minute; at times however it has intermitted, and totally stopt for some seconds; but this irregularity has of late been less frequent.

At the beginning of dinner, a spoonful or two of soup, or the first morsel he eats, particularly if salt or spiced, produces a profuse perspiration on the head, though at other times neither heat nor exercise produces perspiration.

No prescription or advice will be attended to, the materials or rationale of which are concealed. Z.

*** I have to state that the preceding premium is offered by a gentleman of honour, and that I will take care to forward to his address any communications which medical gentlemen may wish to send to him. Let them be sent *sealed*, and without expense, in an envelope, stating that the inclosure relates to this premium.—A. T.

XXII. *Observations on the Advantages arising from the Use of the Wire-gauze Safety-lamp, commonly called The Davy. By Mr.* John Buddle.

Having observed in some of the periodical publications certain remarks on Sir H. Davy's lamp, which in my mind appear to have originated in motives unconnected with truth and the improvement of science, I feel myself called upon to do an act of justice

justice to the merit of the invention, in a public statement of its great utility and extensive use in the coal-mines of this country.

It is not to be expected that any great discovery should be brought to light without subjecting its author to the envy and insult of dabblers in science. Enough of rancour and spleen have certainly been shown in this case. But I shall not take up my time with enumerating the names and the misrepresentations of those officious and mischievous persons, who have endeavoured to persuade the world that there is little either of merit or utility in this invention: their motives are too apparent not to be distinctly seen through, by all observing and liberal-minded men. I must, however, in justice to myself, most positively contradict a calumny which has been industriously circulated, and I feel the greatest reluctance in noticing this vile report, because it is a libel upon the honour and integrity of Sir H. Davy, as well as myself. It has been reported that I had clandestinely communicated certain ideas of Mr. Geo. Stevenson on a safety-lamp, to Sir H. Davy,—than which there never was a more gross, a more unfounded, or more *malignant* statement circulated. I think it right to notice this in the strongest terms, lest the falsehood should finally pass for a truth. It should have been contradicted sooner, had I not thought it of a nature too ridiculous to be received with credit: even at present I could not have been induced to notice so contemptible a slander, were I not assured that it still continues to be propagated.

The wire gauze lamp, however, is in use, and the benefits that it offers to mankind must in time subdue the uneasy sensations of envy and ingratitude that it has excited. During the last ten months it has been extensively employed in all the collieries under my inspection; and it gives me the highest pleasure to be able to state, that during that time not the slightest accident by fire has occurred from its use, though several hundreds of lamps are daily employed.

In the parts of mines where fire-damp prevails, the surveys and inspections are now carried on by the light of the lamp without apprehension of danger from explosion; for experience has shown us, that, with the caution of keeping it in proper repair, it is absolutely safe; and for the truth of this, I appeal to all my professional brethren who have had occasion to use it, without fear of contradiction.

The colliers never hesitate a moment to take it into any respirable part of a mine, however much it may be charged with fire-damp; for, whenever it appears that the air, either from discharges of gas, or from casual interruptions of the circulating current, becomes explosive,—only give the collier his *Davy*, and he goes to his occupation with the same confidence in this impure

pure atmosphere, that he would do in any other situation with a candle.

There has been much quibbling about the *perfect* safety of the wire-gauze lamp. I scarcely know how the words perfect safety can apply to any invention for the preservation of human life; but when we have seen some hundreds of the wire-gauze lamps in daily use for several months past, in all varieties of explosive mixture, in the most dangerous mines of this country, without the slightest accident occurring, it seems only reasonable to infer, that they approximate as nearly to perfect safety as any thing of human contrivance or manufacture can be expected to do.

It would, however, be quite unreasonable to expect that accidents are never to happen, where the wire-gauze lamps are used; for it must always be remembered, that, setting aside the chance of their being damaged by some of the casualties incidental to coal mining, they are to be intrusted to the management of a body of men amongst whom negligent individuals will be found, who may use damaged lamps, or expose the naked flame to the fire-damp, in spite of the utmost vigilance of the overmen and inspectors of the mines. Instances of great negligence have occurred, fortunately without any ill consequences—always with the dismissal of the offender from his employment; but it would be absurd to condemn the lamp, or even to quibble upon its want of safety, on this account.

Independent of the principle of safety from explosion, which the wire-gauze affords, I do not hesitate to assert its superiority over every other medium which has yet been contrived for emitting light in safety-lamps, inasmuch as its flexibility enables it to sustain very great violence. This I say of the common gauze, which has hitherto been used. The twilled gauze, which Sir H. has lately adopted, is of such strength as in a great measure to obviate every objection on the score of weakness, or liability to be damaged. It is a substance which unites the great advantages of durability, and transmitting sufficient light. Of the wire-gauze lamp, therefore, whether with plain or twilled gauze, my decided opinion is, that it so greatly excels all other lamps which I have hitherto seen, with respect to safety, convenience, and light, that I should not think myself justified in using any of them while I possess the highly superior advantages of it. I do not wish to speak invidiously of the labours of other men, but would have it understood, that I have been influenced in my conduct in this affair by rational and disinterested motives; and (when the responsibility I am under is duly considered) no one can certainly disbelieve me when I say, by conscientious and moral considerations.

Both safety apertures and tubes occurred to Sir H. Davy, in the

the very outset of his inquiries; but besides the complexity of tubes, and the difficulty of admitting sufficient air to feed the flame by apertures, glass, or some other transparent substance, had to be employed, as a medium for the transmission of light, and these are imminently liable to accident. He therefore relinquished these discoveries, and adopted cylinders of wire-gauze, on account of their combining the advantages of transparent substances without being liable to their inconveniences *; and the same reasons, I have no doubt, have decided others in the choice of the wire-gauze lamp.

Great pains have been taken to impress the public mind, that certain viewers of this neighbourhood bruited about the excellence of Sir H. Davy's lamp, and brought it into use in preference to others of some pretended superior merit. The falsehood of this calumny is only equalled by its absurdity. Is it likely, in the name of common sense, that those to whose care the lives of so many of their fellow-creatures are intrusted, and who also risk their own existence daily on the wire-gauze lamp, should have adopted it from any other consideration than that of a thorough conviction of its exceeding every other description of lamp, in safety, simplicity, and utility? It would certainly be expecting too much of human nature to suppose that such a compliment could be paid to any one, let his rank in society or his eminence in science be what they may. Such an idea could only have sprung from the conceited opinions of those closet and fireside viewers, who know little more of a coal-mine than its name, and who cannot be supposed to be competent to sit in judgement on matters in which they are wholly devoid of experience: and it is only from such, that we have ever heard of any objections to the wire-gauze lamp.

No one that has actually the charge of a fiery colliery has hitherto denied the safety of this lamp, or set it on a level with any that have been constructed on modifications of its principles. It can scarcely happen that unprejudiced and practical men can have any doubts on the subject; and though much abuse has been bestowed on our profession, for ignorance, stupidity, aversion to improvement, and the like, I am, however, enabled to state, that, at least in the present instance, the viewers of this country have readily adopted a great improvement in the science of mining. The schemes of visionaries and theorists may have been treated with indifference; but real improvements have always, I believe, been readily patronized by the coal owners of this country.

Wallsend Colliery, January 13, 1817. JOHN BUDDLE.

* See Sir H. Davy's communications on the subject to the Royal Society.

XXIII. *On*

XXIII. *On the pretended Priority of Mr.* Stevenson's *Safe-lamp. By* W. P. Knight, *Esq.*

To Mr. Tilloch.

Sir, — In my letter written to you last month, on the very absurd claim advanced by Mr. Brandling, I stated it as my opinion, that with respect to dates so near each other as those quoted by Mr. Hodgson and Mr. Brandling, it was difficult to draw any inference as to priority. In perusing, however, the evidence with more attention, I feel it necessary to alter my opinion. I had turned over the leaves of Dr. Thomas Thomson's Journal, and in his account of the Royal Society for the last year, I found in the notice of Sir H. Davy's papers no mention made of either tubes or safe apertures. On mentioning this to a Fellow of the Royal Society who was present when these papers were read, he informed me that nothing could be more inaccurate than Dr. Thomson's account.

That in the first paper, read Nov. 9, it was distinctly stated that explosion from fire-damp would not pass through small tubes, and that small apertures were insisted upon in the safe-lamp; and that the great object of the second paper, read Nov. 16, was to enforce the use of tubes or safe apertures. The same gentleman mentioned that he had seen a lamp fed with air by four safe tubes in Sir Joseph Banks's library, in the middle of November. On referring back to the Philosophical Magazine, I find safe apertures and tubes distinctly mentioned as forming leading circumstances in Sir H. Davy's papers.

Now the Rev. John Hodgson states, that he publicly communicated to various persons Sir H. Davy's discovery, that explosions from fire-damp would not pass through tubes of small diameter, on Nov. 2d and 6th, that Sir H. Davy had made a safe-lamp, and that this was mentioned to one of Mr. George Stevenson's employers; yet Mr. Stevenson shows no lamp with small tubes till Nov. 21. Before this time he had made his experiments with a tube and a slider. Will any person believe that Mr. G. Stevenson did not endeavour to profit by what he had heard Sir H. Davy had done? Besides, if he had an independent claim, why was it not made at this time? When Mr. G. Stevenson's third lamp with holes was brought forward Dec. 5th; this was stated to be the safe-lamp* that he had made before Sir H. Davy's researches were published. But I must conclude so evident a case.

Can Mr. W. Brandling be serious, or is he not attacking ironically

* See Phil. Mag. for December 1816.

nically Dr. Clanny, and J. H. H. Holmes? Dr. Clanny in his paper on the original safe-lamps, the steam safety-lamps, and the gas-light lamp, states boldly that he was acquainted with the circumstance that explosion would not pass through small tubes; and J. H. H. Holmes, in a libellous advertisement published in the Scotch and Newcastle papers, accused Sir H. Davy of stealing every thing from Dr. Clanny, except the Doctor's principle of security. I am, sir, respectfully yours,

Chelsea, Jan. 25, 1817. W. P. KNIGHT.

XXIV. *On the Laws of Terrestrial Magnetism in different Latitudes. By M.* BIOT; *with Notes by* T. S. EVANS, *LL.D. of Christ's Hospital.*

[Concluded from p. 16.]

AFTER having thus explained all that is at present known, on the direction of these two magnetic forces in different parts of the earth, it remains for us to consider their *absolute intensity*. This has been much less attended to than the variation and dip; which, no doubt, arises from its being more difficult to measure with exactness. In this department, I know of no accurate observations except those that M. Humboldt has made in his celebrated Voyages and Travels, and those which M. Rössel made in the expedition of Admiral d'Entrecasteaux*.

The

* It has already been mentioned, that the first person who noticed the number of vibrations which a needle makes in a stated time, was our ingenious countryman, Mr. George Graham, whose experiments are related at full length in the Philosophical Transactions for 1723, No. 389; or, the old Abridgment, vol. vi. part ii. page 280. He began to observe, when the needle vibrated 10° on each side of zero. The following are the times in which 100 vibrations were performed.

	Vibrations.	Time.		Vibrations.	Time.
1723. April 1	50	3′ 2″	1723. May 20	50	3′ 11″
	50	2 45		50	3 1
2	50	3 3	repeated	50	2′ 38
	50	2 43		50	2 23
3	50	2 52	do.	50	2 38
	50	2 39		50	2 20
repeated	50	2 53	21	50	2 41
	50	2 35		50	2 23
4	50	2 54	23	50	2 40
	50	2 30		50	2 27
28	50	2 48	25	50	2 41
	50	2 16		50	2 30
			27	50	2 41
				50	2 28

The

The researches of M. Humboldt on this subject have brought to light a very remarkable phænomenon, which is, *the general increase of the intensity in proceeding from the magnetic equator towards the poles.*

The same compass which at the departure of M. Humboldt gave 245 oscillations in ten minutes at Paris, gave only 211 at Peru; and it has constantly varied in the same direction, that is to say, the number of oscillations has always diminished in approaching the magnetic equator, and always increased in receding from it towards the north. These differences cannot be attributed to a diminution of the magnetic strength of the compass; nor can we suppose it would be weakened by the effect of time and heat: for, after a residence of three years in the warmest country of the earth, this compass again gave as rapid oscillations at Mexico as at Paris. Lastly, M. Humboldt neglected nothing in his observations to ensure exactness: and they have since been confirmed by the results which he has found in making the needle oscillate successively in the magnetic meridian, and in the plane at right angles to it: for the dip obtained from these data, agrees perfectly with that by experiment, although he did not then know the connexion between these elements which has since been pointed out by M. Laplace. The exactness of these observations never having been called in question, the truth of the consequences derived from them must also be admitted: and this is the increase of the terrestrial magnetic force, in proceeding from the magnetic equator towards its poles. The experiments made by M. Rössel, at Brest and in New Holland, lead also to the same conclusion.

The explanation just given of our knowledge of the magnetism of the earth, is sufficient to show how very imperfect it is. Ignorant as we are of an immensity of essential data, principally with respect to the variation, we cannot yet expect to arrive at its true causes. We can therefore only seek for some empiric laws, which, by embracing the greatest possible number of facts, may bring before us their numeric relations, and point out the principal elements on which to rest the observations.

I have already mentioned, that a great portion of the observed dips, especially in those parts of the globe where the magnetic

The mean of the first 50 vibrations of all the thirteen experiments is 2′ 49″,4. The mean of the second 50 is 2′ 32″,7: consequently those that receded farthest from zero required 16,″7 of time more than the last, and each 100 vibrations on an average were made in 5′ 22″,1. The difference between these times, shows how necessary it is to begin counting the vibrations when the needle reaches some stated number of degrees on each side the magnetic north: for otherwise no comparison can be made between the number of vibrations in different latitudes.

equator

equator is circular, might be represented very exactly, by the action of two magnetic centres placed at a small distance one from the other, near the centre of the earth. Both M. Humboldt and myself have been led to this result in the work which I have mentioned above, and our Memoir was already published when I learned that the celebrated astronomer Mayer had also arrived at the same conclusion, in discussing the dips known in his time: and that he even used it to represent the variations, in a Memoir read to the Society of Gottingen, but never printed*. The son of this great astronomer having been so obliging as to send me an extract from it, I have been enabled to convince myself of this identity: and I also found that Mayer had discovered, in an experimental manner, that the law of magnetic attractions is reciprocally as the square of the distance.

This consequence, which we have both deduced from elements so different, appears to indicate something more than a merely empiric law. It is therefore necessary to examine it a little closer. First, it is easy to see that a single magnet placed at the centre of the earth could not produce all the phænomena; for then the magnetic equator would be a great circle perpendicular to the right line drawn by the two centres of action, and there would not result from it the inflexion which we have observed in the South Sea. In other respects, such a magnet, in whatever way it might be placed, would necessarily give symmetric phænomena on both sides the plane drawn by its two centres and by the centre of the earth, a symmetry which is no way conformable to the facts observed, especially in the South Sea and the continent of Asia.

Not being able to adopt this simple idea, let us endeavour to depart from it as little as possible; and since we have found that it represents the observations made in Europe and in the Atlantic Ocean sufficiently well, let us try to make such a modification that it may be scarcely sensible in this part of the globe, and yet that it shall be pretty large in the opposite part, where the magnetic equator suddenly experiences its inflexion. We shall arrive at this by placing a second excentric magnet near this point, whose position and relative energy shall be determined in such a way as to agree with the observations. Now, by making the calculation, we find it sufficient to give a very small force to

* This circumstance is mentioned by Professor Robison, at the end of the article *Magnetism*, in the Supplement to the third edition of the *Encyclopædia Britannica*. He also gives the results of Mayer's calculations as they were first published by Lichtenburg in his edition of Exliben's Elements of Natural Philosophy, 1784. The article here referred to, and that of *Variation* in the body of the work, contain, perhaps, the most complete Theory of Magnetism that has hitherto been published.

this magnet, so as to cause the anomalies which occur on this side of the globe to disappear, and also to make the small dips that have been observed to take place in the southern part of the South Sea, agree with the large dips which take place in North America. By thus dividing some other secondary centres in the points of the globe where the irregularities of the variations appear most extraordinary, it is probable that we should end, by representing all of them with exactness, as also the dips and intensities. Thus it is in the system of the world: the principal motion produced by the action of the sun, is modified by the perturbations that are caused by the small masses of the planets. But as it is necessary to know the places of these masses, in order to calculate their influence, so likewise it is necessary that the most accurate observations should point out the position of the different secondary magnetic centres, before we can compute their effects.

Is the central magnetic action, indicated by these phænomena with so much probability, really produced by a magnetic nucleus contained in the interior of the terrestrial globe; or, is it not the principal resultant of all the magnetic particles disseminated in its substance? Of this we are ignorant: but the last supposition appears to be the most probable. The secondary centres would then be determined by some local attractions becoming preponderant. And, indeed, the observations show in a way not to be doubted, that the general system of dips, of variations, and of magnetic intensities, is very sensibly modified, sometimes in a sudden and irregular manner, by the neighbourhood of great chains of mountains. This appears to be confirmed by the singular inflexion which the magnetic equator experiences near the numerous archipelagos of the South Sea. It is well known, indeed, that the islands with which this sea is scattered, are only the summits of very high mountains which elevate themselves quite to a peak from the midst of an ocean where we can find no bottom. If the madrepores, of which they appear to be composed, only form a thin layer, and if, as has been supposed by some very able naturalists, the remainder of their mass has been produced by the action of subterraneous fires, the system of these islands would form the most extensive volcanic chain that there is on the surface of the globe. Then, all the irregularities produced by this system in the general laws of terrestrial magnetism, would have nothing in them but what is simple, and conformable to what is observed in volcanic countries*. For the action of subterraneous fires must necessarily

* If the great magnet of the earth be like many loadstones that are to be met with, and have more than two poles, as the ingenious author supposes, these

necessarily change the chemical state and the natural arrangement of the ferruginous parts, in places where it exercises itself; which changes cannot be made without affecting the direction of the magnetic needle, and modifying the general action of the globe in those points. There are, indeed, several examples of these alterations which have happened suddenly; and M. Humboldt has observed similar ones at Peru, after a great earthquake*. It is therefore possible, that the particular magnetic centre belonging to the South Sea is owing to similar causes. There exist, without doubt, analogous ones in other countries: and is it not their alterations which during the last two hundred years have produced these changes of variation of the compass; which changes are so irregular, that it has hitherto been impossible to find any law for them, but which, by this very irregularity, appear to announce that they are not the effect of an uniform and constant cause? According to this idea, nothing in Europe would oblige the compass to return towards the east; and indeed, since it has ceased to decline to the west, it has not been observed to retrograde by any sensible quantity: so that, according to the observations hitherto made, it is impossible to decide whether it will ever return or not.

The magnetic action of the terrestrial globe is not confined

these will act on each other, and gradually change the forces with which they attract, and thereby influence the variation of the compass. This action will however, in time, bring the whole to an equilibrium; and as two poles appear to be the natural and most simple state, to this it will always have a tendency to return, although the internal changes that take place in the bowels of the earth, arising from chemical causes, may frequently counteract and retard this operation. The metallic substances which the earth contains near its surface are well known to be continually altering their nature: and if the variation of the compass depend at all upon the changes of these substances, as is commonly supposed, it is evident, these changes take place much more rapidly than is generally believed.

* Father de la Torré observed in Italy, during the great eruption of Mount Vesuvius, that the variation was 16° in the morning; at noon it was 14°; and in the evening it was 10°, and it continued in that state till the lava grew so cold as no longer to emit any light in the night, after which it slowly increased to 13½ degrees, where it remained.

Daniel Bernouilli found the needle change its position 45′ by an earthquake.

Professor Muller, at Manheim, observed also that the variation of the needle in that place was greatly affected by the earthquake in Calabria. Such streams of lava as flowed from Hecla in the last dreadful eruption, must have made a transference of magnetic matter that would considerably affect the needle: but no observations seem to have been made on the occasion; for we know that common iron-stone, which has no effect on the needle, will, by mere cementation with any inflammable substance, become magnetic. In this way Dr. Knight sometimes made artificial loadstones. —*Ency. Brit.*

to its interior, or to its surface; it extends also into space, as has been proved by Gay Lussac and myself, in an aërostatic ascent. It even appears, according to our observations, that the intensity of this action decreases slowly in proportion as we remove from the terrestrial surface; for we did not find any sensible diminution at the height to which we were elevated. Probably its diminution follows the general law of magnetic actions; that is, the inverse ratio of the square of the distance*: if so, it will extend indefinitely into space. Analogy leads us to think that the moon, the sun, and the other celestial bodies, are endowed with similar actions; and the more so, as the composition of aëroliths that have fallen on our globe have shown us that the heavenly bodies contain, in like manner, magnetic substances, such as nickel and iron †. The magnetic actions of all these bodies must, therefore, according to their positions and distances, influence the direction of the magnetic needle on the earth's surface, as well as the absolute intensity of the directing force: and as these positions and distances are continually changing, by the effect of the motion of the earth and of all the planets, it must follow that there will also be perpetual alterations in the magnetic forces. For example: If the magnetic action of the sun and of the moon be sensible, the motion of rotation of the earth round itself, and its motion of revolution round the sun, must produce diurnal and annual oscillations in the magnetic needle. Now, not only do such motions exist, but their periods, having been established by a long series of observations, agree with the cause which we have just pointed out. At Paris, according to M. Cassini, the maximum of the diurnal variation ‡ appears to take place between noon and three in the afternoon; then the needle becomes stationary; it then approaches towards the terrestrial meridian, until about eight in the evening; then it stops, and remains stationary during the whole of the night. But in the morning, about eight, it begins again to recede from the meridian. If this second motion remove

* This was the law of magnetic attraction discovered by Coulomb; but Dr. Brook Taylor found by experiments which he made to ascertain it, that at small distances it was nearly in the inverse ratio of the square of the distance: and further off it was inversely as the cube of the distance, or even more than that.—Phil. Transactions, vol. xxix.

† It is here supposed that these stones come to us from some of the heavenly bodies, which has not yet been proved.

‡ The diurnal variation was first observed by Mr. George Graham in March, April, and May 1722, and published in the Transactions of the Royal Society, No. 383. The greatest variation westward which he found was 14° 45′, and the least 13° 50′, by a mean of about 1000 observations. Generally speaking, it was seldom less than 14°, or greater than 14° 35′. But the most detailed account of it is given by Mr. Canton, in the Philosophical

move it further than the evening before, it follows that the variation is increasing from one day to the next; if the contrary, it is decreasing. The greatest diurnal variations generally take place during the months of April, May, June, and July; that is to say, between the two equinoxes of spring and autumn. They are at Paris from 13 to 16 minutes. The least are from 8 to 10 minutes, and they take place during the remainder of the year. Now, if we compare the analogous positions of the needle on different days, but at the same hours, in order to have its general progress, we find that, from the spring equinox to the following summer solstice, the variation is decreasing; and that it is increasing all the rest of the year, that is, from the summer solstice to the following spring equinox (as is represented in fig. 4). For our knowledge of these periods we are indebted to M. Cassini, who has established them by observations made during eight years at the Observatory at Paris.

Lastly; numerous observations prove that the magnetic needle is subject to sudden and accidental alterations, which depend on the appearance of those luminous meteors called *auroræ bo-*

phical Transactions, vol. li. p. 399, from whose observations it appears that, although there be great irregularities in this diurnal change of position of the needle, yet there is a certain average which is kept up with considerable steadiness. The following table shows the average of greatest daily change of position in the different months of the year, observed in Mr. Canton's house in Spital Square, London, in 1759.

	′ ″		′ ″		′ ″
January	7 8	May	13 0	September	11 43
February	8 58	June	13 21	October	10 36
March	11 27	July	13 14	November	8 9
April	12 26	August	12 19	December	6 58

Mr. Canton attempts to account for these changes by observing that the force of a magnet is weakened by heat. A small magnet being placed near a compass needle, and E.N.E. from it, so as to make it deflect 45° from the natural position, the magnet was covered with a brass vessel, into which hot water was poured. The needle gradually receded from the magnet 3-4ths of a degree, and returned gradually to its place, as the water cooled. The parts of the earth to the eastward are first heated in the morning, and therefore the magnetic force of the earth is weakened, and the needle is made to move to the westward: but as the sun warms the western side of the earth in the afternoon, the needle must move in a contrary direction. Æpinus supposes that the sun acts on the earth as a magnet acts on a piece of soft iron; and in the morning propels the fluid into the N.W. parts. The needle directs itself to this constipated fluid, and therefore it points to the eastward of the magnetic north in the afternoon. Neither of these explanations will account for the great diversity of the diurnal variations in different places, which are so great, that Professor Robison thinks, we can hardly ascribe the diurnal variation to any change in the magnetism of the primitive terrestrial magnet, and must rather look for its cause in local circumstances.

 reales.

*reales**. The absolute cause of this correspondence between them is unknown, as well as that of the *aurora borealis* itself. The influence of this meteor on the needle is commonly of short continuance; for, after being quickly agitated as long as this manifests itself, the needle then returns to its usual position, and proceeds again in the accustomed order of its motions: but it also happens occasionally that it experiences a durable removal from its place, some instances of which have been observed by M. Cassini.

To measure these variations, whether diurnal or annual, Coulomb used a steel needle, suspended to an assemblage of single filaments of silk as taken from the natural investment of the silkworm, and just sufficient to support it. At the two extremities of the needle he fixed two metal arcs, carrying very fine circular divisions. The whole apparatus was inclosed in a box glazed above, in order that the needle might be observed without being agitated by currents of air. Two microscopes fixed on the box, and furnished with hair micrometers, could each be directed to the divided arcs carried by the needle, and thus showed its smallest motions. M. de Prony used for the same purpose a long magnetic needle, carrying on its length a telescope which moved with it; the motion of which was observed on a distant mark. M. Humboldt, who made a number of observations with this apparatus, considers it very exact.

It is important, for the future progress of natural philosophy, that we should determine with accuracy the present *intensity* of terrestrial magnetism, in the same manner as the weight of the atmosphere and the actual temperature of different climates have been ascertained. By repeating the same observations after some centuries, it would be known whether the magnetic force varies in its energy, at it is certain it has varied in its two directions.

The first method which presents itself to the mind, is to observe the variation, the dip, and the intensity, immediately, by means of three needles appropriated to this purpose, and then to preserve them carefully for further trials from time to time. As they are liable to lose their magnetism in this interval, they might be restored to the same state by a fresh process, using for this purpose bars well combined together, according to *the method of*

* "The *aurora borealis* is observed in Europe to disturb the needle exceedingly; sometimes drawing it several degrees from its position. It is always found to increase its deviation from the meridian, and makes the needle point more westerly. This disturbance sometimes amounts to six or seven degrees, and is generally observed to be greatest when the *aurora borealis* is most remarkable. No needle but a magnetic one is affected by the *aurora borealis;* we may therefore conclude that there is some natural connexion between this meteor and magnetism."—*Robison.*

the

*the double touch**. Indeed, in the application of this process, the needles, by the influence of the extreme bars, are brought instantaneously to a much stronger degree of magnetism than that which they could preserve when they are left to themselves; so that, if their internal constitution remain the same, the degree of magnetism in which they are fixed must also remain the same, or at least can only experience that degree of alteration which arises from a change of intensity in the magnetic force of the globe. This method might be rendered much more certain by keeping thus a stated number of well tried needles, the separate effects of which might be ascertained at any previous stated epoch. For if, in trying them again at any other time, it should be found that they have preserved among them their original properties, we might conclude with certainty that they have not been altered in their constitution, and consequently the observation of their absolute energies would determine the real state of the magnetic force.

But the adoption of this method requires the preservation of the needles, and the assurance of their identity. This care might be dispensed with, if we could find a method of fabricating two needles capable of being accurately compared together at any time. For this purpose we must not think of employing *steel*, which, being a mixture of carbon and iron, is necessarily variable in its proportions. But this defect might be supplied, if we could procure some wire of perfectly pure soft iron by chemical means. For, according to the experiments of Coulomb, which we have already mentioned, twisting gives such a degree of density and elasticity to iron, that it takes magnetism nearly as well as steel, and retains it equally as long: it is, therefore, only necessary to regulate the quantity of twisting. Now, this is easily done by taking wires of an assigned length and size, and measuring the number of turns which they are twisted by means of a micrometer. Each of these wires should then be magnetized to saturation, and a certain number of them should be collected together, so as to form a bundle, the directing force of which should be measured, either by a magnetic balance or by the method of oscillations. All the difficulty is thus reduced to the procuring of pure iron, and this difficulty belongs solely to chemistry.

If in any given place a piece of soft iron, such as a key for example, be put in the direction of the magnetic dip; the key presently acquires two poles, which act on the magnetic needle in contrary directions. Turn the key about, and its poles will change, and the same effect may be reproduced.

* See Supplement to the 3d edition of the *Ency. Brit.* art. *Magnetism*, No. 59; and for a list of all the best writers on this subject, see the end of that article, No. 89; or Dr. Thomas Young's very valuable Treatise of Natural Philosophy, vol. ii. page 437.

XXV. *Answer to Mr.* John Farey *Sen. and to Dr.* John Murray. *By* J. Murray, *Esq.*

To Mr. Tilloch.

Sir,—My name being introduced by Mr. John Farey senior, and Dr. John Murray, in your last number, I am peremptorily called upon for an early reply.—That answer is now submitted.

A contest regarding the priority of an invention may certainly be carried on without asperity, for the object is merely to ascertain a fact; and it is equally obvious that few would be found so silly as to wish to have the merit of inventing a thing of no use. Mr. Farey has nothing to do with this contest: his is of a different kind. He aims at inducing the neglect, the disuse, and the contempt of the invention of the safety-lamp. Now if others could with him believe it useless, and be brought to consider it not merely as a matter of indifference, but as criminal, to defend men from the danger of being destroyed by explosions of fire-damp, they might then talk of it with much composure. But others do not take the same view of it as Mr. Farey, and he of all men has the least right to complain if the charges which he brings against others should, sometimes, be met by terms which ought not to appear in the nicely-balanced language of courtesy. I am unconscious of having offended Mr. Longmire. If I have, I entreat his forgiveness. The good and generous mind will impute it to an honest warmth in favour of a discovery the most important that has ever risen upon the horizon of existence: and when I consider "what it has done for thousands," I am absorbed and lost in the contemplation. I may be called an enthusiast: Indeed I am—I assume not the character of a man of science; but I trust Mr. Farey senior will not deny me the appellation of a *philosopher*. I shall be content to follow (though at an immeasurable distance between) onward in that path of research which has been illuminated by the discoveries of Sir H. Davy.

The recommendation of an *occasional* use of the safe-lamp is one which will not be followed; nor can I conceive any thing more absurd or ridiculous than to employ the instrument on a Monday, and *then hang it up* the week through. The fatal effects of such a procedure have been recently awfully exemplified in the last colliery which I descended. I *entreated* Mr. Roscoe to the continued use of Sir H. Davy's safe-lamp—to be uniform and universal. The neglect of the precaution has accelerated that event which I hope will be a fence against any impressions from the doctrines of Mr. Farey senior. Sir H. Davy's lamp

lamp was used in the Bagilt colliery, and some were ordered from Mr. Newman of Lisle-street, by Mr. Roscoe, to whom I submitted Mr. N.'s address; but such has been either only occasional, or used in such parts of the mine as were too alarming to be disregarded. I would not for a moment be understood to impeach either the prudence or precaution of Mr. Edward Roscoe, whose attention and care are highly honourable to him.

Mr. Farey senior cannot say I took him by surprise in the letter inserted in "The Carlisle Patriot," as I dispatched to him the paper which contained it. For what purpose he has kept it out of sight for about a *twelvemonth*, I cannot even conjecture; nor can I divine for what end it is now introduced. I may here state, that at this period I had not seen Sir H. Davy's safe-lamp, nor was aware of the extent of its application. It was then in infancy, and I only obtained a confused and indistinct idea of its structure and properties through the medium of the press; but when I received one, and passed it through explosive atmospheres, and saw its simplicity and its efficacy, and proved its "mightiness," I became a convert, and believed. All other lamps associated with this, notwithstanding Dr. Murray's self-complacent opinion, are bubbles in the comparison—"Hyperion to a satyr." Mr. Farey's latter paragraph is pitiful, very pitiful.

Dr. John Murray commences his paper with observing, that I was not aware of its being taken from The Transactions of the Royal Society of Edinburgh. There is nothing to warrant this conclusion—I questioned the propriety of making this a *leading article* in the Annals of Philosophy; and as for the *violence* offered to Dr. John Murray, judge ye:—In reply to the article in the "Edinburgh Star," I explained and gave in answer a satisfactory rejoinder. In concluding, I remarked—"It must be evident that the Doctor, feeling himself anticipated, is sore upon the subject. This is the *punctum saliens*." Such is the *violence* I have done to the person and character of Dr. Murray! Confident that my reply was most conclusive, Dr. J. Murray anticipates in his paper *an answer*; but he excuses himself from noticing it. This is an easy way of getting rid of his dilemma. Why does Dr. Murray confine himself to remarks respecting the structure of the CHIMNEY, which I admit in the first case was not so simple as that afterwards adopted, and described in your last number? The other suggestions are quite independent of the question, and have nothing to do whatever with the position, "A lamp might be made air-tight and fed by a flexible tube from the floor of the mine; the carburetted hydrogen being lighter and ascending would occupy the upper part, and thus could not enter the tube." And yet, with this language before him, he *affects to deny* that I have anticipated him in suggesting the structure of

of an air-tight lamp founded on the specific levity of the fire-damp, to be fed by air contiguous to the floor of the mine;—and directs his objections against *the other suggestions*, that are insulated and unconnected. It is strange that every other person who reads the paragraph cedes the priority to me. And though Dr. Murray may accuse Sir H. Davy and the Rev. Mr. Hodgson of collusion—Psha! psha! he surely cannot apply this to Mr. Knight and Philalethes in your last number, who HAPPEN to be of the same opinion. Respecting the *other propositions*, which the Doctor pronounces untenable, because not sufficiently explanatory, I shall prove to your decided conviction in a future number, that they are founded on well-known hydro-pneumatic laws; meantime this distinct feature belongs not to our question. I do not comprehend what Dr. Murray means by the expression "all that I was called on to do." For my own part, I was a volunteer in the cause of humanity, and I am sure Sir H. Davy was. Dr. J. Murray observes, that "the plan in all its extent is preferable to any that has yet been brought forward." Really this is too bad.

Let me honestly assure Mr. Knight that I do not claim, nor have I claimed, the priority of constructing a lamp on the principle of a diminished atmosphere. God forbid that I should be thought possessed of the wish to rob Sir H. Davy of a single leaf of the wreath of which he is the wearer! No: I shall rejoice to see it flourish:—I'd

> "Rather be the thing that crawls
> Upon the dungeon wall."

Heaven itself will smile upon and consecrate it. His last interesting discovery is justly entitled to the appellation το καλον, or *the beautiful.*

Nobly as Sir Humphry Davy has served the sacred cause of humanity, and brilliant as have been his discoveries, I must express it as matter of astonishment and regret that no public testimony of national gratitude has been voted him. As a citizen of the world, and appreciating the talents of the distinguished individual who has unfolded "the portals of worlds unknown," I would humbly propose that the friends and admirers of Sir H. Davy should award some acknowledgement expressive of their gratitude and esteem, and to commemorate in a lively and lasting manner how highly they honour true taste and feeling when properly directed, and how much they value those exalted talents by which the lives of myriads of human beings are protected and saved from destruction—a scene at which my best feelings glow even in anticipation, and when only discerned through the valley of those shadows which bound the prospect of

of futurity. I like not that cold philosophy whose end and aim is Stoicism. For my own part, I trust I shall be permitted to add "my stone to the cairn:" and, though feeble the tribute, it will be honestly and cheerfully bestowed.

I hope it will not be said that the question rests with the proprietors of coal-mines. This narrow and contemptible idea can only be entertained by those whose puerile and contracted views are mean and despicable. On the great and glorious cause in question, there can be but ONE voice. Let it be expressed, and the great and good will be glad to meet the appeal, and honour and sanction it.—I fondly trust the suggestion will not be despised because it emanates from so humble an individual as myself. I feel somewhat gratified that, touched with the awful recital of the destruction of human beings, I was among the earliest in the field;—and when the distinctions of fortune shall sink into oblivion, the name of DAVY shall live, and be associated with that of Newton and other benefactors of the human race, and be venerated, and ever named with gratitude.

I always am, sir,

Your faithful and obedient servant,

Surry Institution, Feb. 3. 1817. J. MURRAY.

P. S.—Dr. John Murray is required to *interpret* the following passage from page 154 of my "Elements of Chemical Science," published 20th of June 1815, and FIVE MONTHS before his paper was read before the Royal Society of Edinburgh, and not confine himself to a *violent* invective against *ex parte* and *garbled* extracts from suggestions of an unconnected and independent description.

"A pipe might supply an air-tight lamp from the mine, and the orifice of the tube receive the supply from the stratum of air contiguous to the floor. The carburetted hydrogen being lighter, and ascending, would occupy the upper part, and thus could not enter the tube." J. M.

XXVI. *On the Union of Copper with Iron, &c. By Mr.* P. N. JOHNSON.

To Mr. Tilloch.

SIR, — YOU have several times favoured me by inserting in your valuable Magazine the results of experiments I have made on various substances, exhibiting effects very different from what would be suggested by mere theoretical speculation.

I now wish particularly to call the attention of your readers to the combination of copper with iron, which, although stated by

by many writers on metallurgy to be capable of uniting in an INDIRECT way, is yet by most operative men, as casters of copper, and others, positively denied to have any such capacity.

I have had my attention directed to this subject, by being summoned as an evidence in a cause Smith *v.* Frost. Mr. Smith, who uses copper pans to boil the ingredients for making a green pigment for painting and dyeing, had been recommended to make use of cast pans, to save the expense of wrought ones; but these not answering his purpose, he employed me to inspect and give my opinion on them. In my experiments I proved the presence of tin and iron; the latter was in a very minute quantity: but from the proportion of the former the contract was supposed to be void. The opposite party however, being copper-smiths or casters, positively asserted the impossibility of the union of copper and iron. And finding, on inquiring of several persons in the same way of business, that a similar opinion generally prevailed, I resolved on making some experiments to prove how far it was possible to unite these metals.

I first mixed 100 parts of copper with two of iron, covering them with rosin and filling the crucible with powdered charcoal. After being exposed to about 90° of Wedgwood's thermometer for a quarter of an hour, the mixture gave a clean lump of copper not quite so malleable as when unadulterated, and with a redder grain. I then endeavoured to ascertain how much iron the copper would take up, by covering 400 grains of pure copper with iron filings, and filling the crucible as before. The produce was 880 grains, of a large red grain, bubbled in the inside as if occasioned by confined air, with a clean uneven surface, and possessing nearly the malleability of zinc.

The next thing was to prove the existence of the iron by the usual process of analysis. The increased weight, indeed, clearly proved its presence; but I thought the analysis necessary, to prove that only the iron had entered into the composition. As the iron I had used for the foregoing experiments was slightly oxidated, I fancied this might have facilitated the union. I therefore subjected 400 grains of copper covered with black oxide of iron (the crucible filled with charcoal) to a strong degree of heat for half an hour. The produce was 526 grains of copper remarkably red, which on analysis nearly answered to the increased weight as metallic iron. I consider the iron to have been the cause of the copper having such a red appearance, from its partially oxidating it: it may perhaps, too, have had the effect of making it more brittle, by separating the particles of metallic copper. I further ascertained that this oxidation greatly facilitated the combination. Having melted 400 grains of pure copper, with a clean bit of thick iron wire, taking care

to

to cover the crucible well, as in the former experiments; the produce was a lump of copper, in the heart of which a bead of steel (containing a proportion of copper) was found inclosed, with some loose bits of steel, on the surface of which a few grains of bad copper were to be perceived. The copper was very malleable, but not so much so as when unadulterated, and containing only 4½ per cent. of iron.

The formation of the steel may of course be accounted for by the crucible being filled with charcoal, to prevent the oxidation of the copper.

Although the union of these two metals is certainly not so perfect as that of other metals, yet I trust that those who credit these simple experiments will abandon the prejudice of there being any impossibility in their combination. For my own satisfaction, I have made several experiments in uniting copper with other metals; and perhaps you may consider them sufficiently interesting to give them also a place in your pages. What more particularly struck my attention, was the effect of arsenic when melted with copper. It altered the colour without increasing the weight of the copper, being no doubt volatilized.

United with two hundredth parts of arsenic, the copper was rendered whiter, softer, and more ductile, but not increased in weight. United with ten hundredth parts of arsenic, the copper, as in the former case, was not increased in weight, but became very white, and not quite so malleable.

It may be necessary however to observe, that I used the glass or oxide of arsenic, which might have facilitated its evolution; although, as the crucibles were filled with charcoal dust, I thought this would have prevented any such effect.

Copper united with two hundredth parts of tin was rendered less malleable, became of a flaky bright when suddenly flattened by hammering, was smooth in the fracture, and had a colour inclining to yellow, and somewhat whitened.

Copper united with two hundredth parts of lead assumed a bright flaky appearance when hammered, and the malleability was much diminished.

Copper united with two hundredth parts of zinc was rendered softer and less ductile, but not so flaky as when united with either tin or lead: the fracture was of a dirty-red colour.

I shall shortly have the pleasure of submitting to your notice the result of my experiments on the combination of some other metals. I remain

Your respectful and obliged servant,

P. N. Johnson,

Maiden-lane, Wood-street. Mineralogist and Assayer.

XXVII. *On*

XXVII. *On the Restoration of Vision, when injured or destroyed in consequence of the Cornea having assumed a conical Form.* By Sir William Adams.

To Mr. Tilloch.

Sir, — Having written and sent the subjoined paper almost too late for insertion in the last number of the Journal of Science and the Arts, and being disappointed in the expectation of seeing a proof sheet, in which it was my intention to have made some important corrections and additions, I now take the liberty of requesting that you will insert it in the next number of your valuable Magazine; by which you will probably prevent a misconception of some of my opinions, and much oblige

Your obedient humble servant,

26, Albemarle-street, Jan. 10, 1817. Wm. Adams.

*On the Restoration of Vision, &c.**

Among the causes producing short sight, is a morbid thickening of the transparent cornea, which has been usually termed conical cornea. One of the first, and, I may add, the best descriptions yet given of it, has been by Dr. Léveillé, an eminent French physician, and translator of Professor Scarpa's work on diseases of the eye, into the French language.

The conical cornea, although a disease not so frequently met with as many other morbid affections of the eye, is yet by no means of rare occurrence; and any curative effort which is capable of being successfully exerted, becomes the more interesting, the advanced stage of the disease (as far as I have been able to learn) having been hitherto considered by authors as incurable.

It is, therefore, highly gratifying, that the resources of art are found competent to afford effectual relief even in this apparently hopeless case, in which, although it is impossible to remove the disease itself, when thus fully formed, yet, by taking away one of the healthy parts of the eye, whose use is similar to that of the diseased growth of the cornea (which does not admit of removal), vision, it will be shown, may be restored nearly to perfection.

The malady in question commences by a morbid growth of the whole substance of the cornea, but more particularly its central part, situated opposite to the pupil, and the patient's degree of short-sight increases in exact proportion to this growth, which takes place without inflammation, and in general without opacity.

* The corrections and additions alluded to in Sir William's Note are incorporated in the subjoined copy.—Edit.

opacity. Its advance, unless arrested by the appropriate medical treatment, is usually slow, but progressive, until at length the cornea, instead of being a regular segment of a sphere, wholly losing its natural curvature, assumes a conical form.

This change of structure produces some curious phænomena in the appearances as well as the uses of the cornea. On examining it in front, it assumes an unusual degree of sparkling brilliancy, nearly resembling crystal, except (as is sometimes the case) where there is an opacity in the apex of the cone. Dr. Léveillé attributes this brilliancy to the cornea's strongly reflecting, instead of transmitting, the rays of light, by which he supposes that the pupil becoming contracted, in a strong light, thereby produces an imperfect and confused sight; but this explanation of the cause of the patient's imperfect vision, it will be hereafter shown, is erroneous.

If the cornea be examined laterally, it will be observed that the thickening gradually increases from its circumference to its centre, where the apex of the cone is usually seated, although, in some instances, I have seen it on one side of the centre. If the cornea be similarly examined, opposite to a strong light, its thickness at the base may in general be traced, while the sugar-loaf form of the apex renders it impossible to be mistaken for any other disease of the eye.

The change which this disease produces, in respect to vision, is very important. Soon after the commencement of the diseased growth, the patient complains of being unable to see objects distinctly, at the usual distance; and his power of vision becomes gradually shortened, in proportion as the disease advances, until at length he is unable to perceive minute objects with any degree of distinctness, however near they may be placed to the eye; and cannot make out large ones when above three or four feet distant. In fact, vision is destroyed in relation to the useful purposes of life, and he becomes nearly as dependent as if totally blind. Indeed, I once saw a young lady labouring under this disease in both eyes, who did not venture to go any where without a guide.

The disease generally begins at the first in one eye, and a similar affection commonly succeeds in the other. I have met with it in almost every stage of life, from a girl of sixteen to an old lady of seventy, and am not aware that it is peculiar to any sex or age, although I have certainly seen it much more frequently in women than in men, and more in young than in old persons.

The opinions generally entertained of the cause of the disease in question, appear to me to have been incorrect, and have necessarily led to an erroneous practice in the attempts which have

have been made for its alleviation. The conical form of the cornea has been attributed to an over-distention of that tunic, occasioned by a superabundant secretion of the aqueous humour, which continually stretching the cornea, has gradually occasioned it to yield to the pressure from within, and thus produced the alteration in its form. To remove this supposed over-distention, it has been usually recommended to evacuate the aqueous humour, by puncturing the cornea, and afterwards to employ pressure, astringent collyria, &c. to prevent its reaccumulation. Experience has, however, shown the total inutility of these modes of practice. The operation of evacuating the aqueous humour has been, in some instances, repeated several times, without any permanent advantage being found to result from it; and although it is an operation neither painful nor difficult to perform, yet it is sometimes dangerous; for if the crystalline lens should be wounded by the instrument with which the puncture is made, cataract will most likely ensue; and I have been informed of a case, where this actually occurred, during the attempt to evacuate the aqueous humour.

Having, at an early period of my practice, been impressed with the opinion, that the conical form assumed by the cornea in this disease, was the effect of a *morbid growth* of that tunic; and that the short-sight experienced by the patient, was to be attributed to its increased refractive power, which, together with that of the crystalline lens, brought the rays of light to a point far short of the retina, it occurred to me, that as it was impossible to remove the morbid growth of the cornea, without rendering it unfit for the transmission of light, a useful degree of vision might be restored by the removal of the crystalline lens. I was the more strongly led to form this conclusion, after having myself tried the experiment of looking through deep convex spectacles, such as are employed by patients after the removal of cataract, which, I found, produced a confusion of sight, very similar to that which I had heard described by persons in whom the cornea had been conical in the extreme degree. I therefore resolved, more than six years since, while surgeon to the West of England Eye Infirmary, instituted at Exeter, to remove the crystalline lens in a case in which that body had become opake, and also affected with conical cornea. Some circumstances, however, prevented the patient, who was a young woman, and a pauper, from being sent to me to the Infirmary. About three years since, another patient, from the country, an old woman, nearly seventy years of age, placed herself under my care, labouring under this disease, accompanied with cataracts, in whom I successfully removed both the cataracts, and had the gratification to find her vision thereby restored to an extent which far surpassed

surpassed my most sanguine expectations. I observed that she was capable of seeing much more distinctly without convex glasses than is usual for persons who have undergone the operation for cataract, while, with a convex glass, she could read small print without any difficulty. Not being able to ascertain the degree of vision which this patient experienced previously to the removal of the cataracts, nor whether the diseased change had been going on in the cornea and crystalline lens at the same time, I necessarily cannot state the exact amount of the benefit which she derived from the operation. This, however, was demonstrated—that, by the removal of the crystalline lens in eyes affected with conical cornea, nearly perfect vision was restored; while it is well known that, in cases of conical cornea where no cataracts exist, vision is usually as imperfect as if the latter malady formed a part of the patient's disease.

The favourable result of this operation fully confirming the opinion which induced me to perform it, I determined at the earliest opportunity to try the effect of removing an healthy crystalline lens, as a remedy for blindness produced by conical cornea. A favourable case presented itself the following year, in a young woman, who, during six years, found her sight gradually decreasing, and, at the expiration of that period, had become so blind, from this disease, as to be unable to continue her employment as a servant, and was in consequence obliged to apply for parochial maintenance. Shortly afterwards she was sent to an Eye Infirmary in London, where receiving no benefit, she was subsequently brought to me, and solicited in the most urgent terms the trial of any practice, which afforded a prospect of restoring her to sight. I carefully examined her eyes, and found that the cornea of both eyes had assumed the conical form in a great degree, attended by a slight opacity in the apex of each cone, but none whatever in the crystalline lens. She could walk without a guide, and could see at three or four feet distance, so as to avoid running against any person, but had entirely lost the power of reading, or perceiving minute objects, however nearly they were placed to the eyes.

I effected the removal of the crystalline lens, by causing it to be absorbed, which method of operating is to be preferred to every other hitherto practised, whether the lens be opake or not, in cases where, like the present, it admits of being freely divided. The patient, however, returned to the country before the eye had entirely recovered from the operation, and I did not see her again until nearly twelve months afterwards, when I was in the highest degree gratified to find her capable of discovering minute objects, and reading the smallest-sized print, without the assistance of a glass, while holding the book at the usual distance of

 ten

ten or twelve inches from the eye, nearly as well as she ever recollects to have done. The usual cataract spectacles for near objects, of two inches and a half focus, confused her sight nearly in the same manner as before the crystalline lens was removed, while with those of nine or ten inch foci, her capability of seeing minute objects was somewhat improved. She saw objects at a distance better without than with any glass I could find; whereas the usual standard for distant vision, after the operation for cataract, is four inches focus. She now neither uses a glass for near nor distant objects, has again returned to service; and a gentleman told me, who has recently seen her, that she accurately described to him an object which was considerably more than a quarter of a mile distant. Twelve months after undergoing this operation, I operated upon the other eye; but she again left town before the eye had recovered itself, and before the lens was entirely absorbed. Previously, however, to her departure, she could read small print with this eye, by the assistance of a convex glass of two and three quarters inches focus, while with one of nine inches focus the sight was greatly improved in viewing distant objects.

As the degree of conical form of the cornea appeared to be the same in both eyes, and as the patient was equally blind in both eyes before the operations, it is a curious circumstance, and deserving notice, that the two eyes should require glasses differing so much in their refractive power. Not being able to obtain any other information from the patient as to the progressive amendment of her vision, during the twelve months she remained in the country, between February 1815 and February 1816, when she underwent the operation upon the second eye, except "that her sight continued to get stronger," (an indefinite mode of expression made use of by poor people in their recovery from almost every species of diseased eye,) I cannot undertake to afford any authentic data for the hypothesis which I venture to offer as an explanation of this phænomenon.

It appears to me that the greater degree of improvement of vision in the eye first operated upon, might be occasioned by the increased susceptibility of the retina from the exercise of that organ, and the power the eye had acquired of adapting itself to see near and distant objects distinctly, during the interval of twelve months the patient was absent; whereas the trial with the two and three quarters convex glass for near objects, and nine for distant ones, on the eye last operated upon, was made only a few weeks after the operation, and even before the attendant inflammatory action had subsided, or the whole of the lens become absorbed. I am led to adopt these opinions from observations made in numerous instances upon persons who have

successfully

successfully undergone the operation for cataract, and who almost uniformly require deeper convex glasses to see distinctly immediately subsequent to the operation, than are necessary afterwards. That the retina and optic nerve become partially insensible from not being exercised, and reacquire their natural susceptibility when again brought into use, are facts which I have so often witnessed, that I judged them sufficiently confirmed to insert them in my practical observations on diseases of the eye; and I have also generally observed, more particularly in poor persons, who from the inconvenience attached to wearing spectacles, appear to feel an objection to their use, that although they are unable to see either near or distant objects immediately after the operation, without glasses, yet, after a time, they acquire the power to a considerable degree of perfection, if they have the patience to do without them. I cannot elucidate these opinions better, than by instancing three cases which have occurred in the extensive opportunities I have had of making similar observations among the patients on whom I have operated for the cataract.

The first was a postillion who had been blind nine years in one eye, and three in the other. Both cataracts were successfully removed by the operation effecting their absorption, and when he resumed his employment as a postillion, he was, from necessity, obliged to wear his spectacles, not being able even to walk without them; but finding that his passengers were frequently apprehensive of their safety, from being driven by a person requiring spectacles, he by degrees left them off altogether in the day, and in the course of twelve months he could drive quite as well without as with them. At night, however, when the rays of light being comparatively few in number, require the most complete concentration upon the retina, to produce a sufficient impression upon that membrane for the purposes of vision, he still derived great advantage from the use of distant sight spectacles. The poor fellow died of pleurisy about two years after the removal of the cataracts. Had he lived, it is probable the susceptibility of the retina would have so far increased, and the adjustive powers of the eye so much improved, that he would have seen even at night, sufficiently well without the use of any glasses.

While at Exeter I effected the removal of cataracts in a young man twenty years of age, who was born with them. The cataracts were originally fluid, but, as is usually the case when this species is suffered to remain for any long period without being operated upon, the fluid part had become absorbed, leaving nothing but an opake capsule containing the grosser parts of the cataract. With the right eye he could see brilliant colours, and

 perceive

perceive light from darkness, but could not discriminate any object; with the left I found him capable of seeing objects at a distance indistinctly, and of distinguishing large-sized letters when held in an oblique direction within two inches of his eyes, and with his back turned to the light. If, however, he attempted to read facing the light, he entirely lost this power; and under the most favourable circumstances, the sphere of vision was so circumscribed, that it did not include above three or four letters at a time. To my surprise, as soon as the eye had recovered from the operation, he was able, without spectacles, to see both near and distant objects with a degree of precision quite unusual without their aid. He returned home at the end of ten weeks (a fortnight only after a second operation, which was performed on one eye), and I did not again see him for about nine months, when I found him capable of reading and writing with *both eyes*, without the assistance of glasses, although with one of them he had never, previous to the operation, been able to discern objects from his birth.

I now engaged him as a footman, being perfectly competent to execute the usual duties attached to this station, except to judge accurately of distances; he could not at this time snuff a candle with certainty, or pour liquids into a small glass. He neither used spectacles to see near or distant objects by night or day, but he always held small ones more than usually near to his eyes when he wished to view them attentively; in doing which he knit his brows, and appeared strongly to exert the powers of the eye. He saw, to all appearance, at as great a distance as any other person, and was fond of viewing extensive prospects. The scenery about the Irish and Scottish lakes seemed to delight him exceedingly. Finding him however incorrigibly idle and inattentive, I was obliged to discharge him at the end of the first year, when his knowledge of distances was so much improved, that in the service where he afterwards lived in the country, he for some time acted as coachman.

Two very extraordinary facts occurred in this case: first, the patient's immediate capability of adjusting his eyes in viewing different distances; secondly, his having so soon established the susceptibility of the retina of that eye in which he had not seen previously to the operation. The first may, I think, be satisfactorily explained by supposing that in his best eye, the power of adaptation had been acquired previously to the performance of the operation; for the crystalline lens having been opake, and nearly absorbed, was not only useless in effecting the natural refraction of the rays of light but also actually impeded them in their progress to the retina, the only passage by which they could make their way to the bottom of the eye, being through the

the edge of the capsule, where it was less opake than in its centre. In the passage of the rays of light through this portion of the capsule, it is evident, very little, if any refraction, could take place, and it therefore required the adjusting powers of the eye to be exercised in order to enable him to see near and distant objects previous to the operation, quite as much, if not more than if the lens had been actually removed.

Thus, then, that power which usually takes a patient six or twelve months to acquire after the removal of the lens in ordinary cases, was possessed in this previously to the performance of the operation.

The small space at the edge of the capsule, through which alone the light was capable of making its way to the retina, explains the necessity of the patient turning his back to the light in order to effect a dilatation of the pupil; for when he faced the light, from the contraction of the pupil, the iris covering the greater part of the space in the capsule through which the light passed, necessarily occasioned a still greater diminution of vision than he previously experienced.

The same cause accounts also for his being obliged to hold small objects in an oblique direction, and for his seeing but three or four letters at a time; for had the part of the capsule through which the light passed, been in the centre instead of at the edge, and also been of a large extent instead of being confined, he would consequently have seen objects straight forwards, and have possessed the power of taking in a much larger extent of vision. The opinion that the latter effect was occasioned by the small space through which the light had to pass, is confirmed by my having generally observed, that where the pupil is naturally small, the field of vision is proportionally circumscribed. In one instance of a patient who came to me after he had undergone a number of operations for cataract, in a public institution, I found the pupil so much contracted as to render it dangerous for him to walk in the streets by day, and entirely to incapacitate him from doing so alone, at night. After forming an artificial pupil of a proper size, he was enabled to see even the nails of his fingers when the arms were extended to the utmost at right angles with the body, nearly as distinctly as he could before I operated upon him, when placed immediately before his eye, and he now sees to walk at night as well as other persons*.

With respect to that eye of my late servant with which he had never seen previous to the operation, and the retina of which so quickly acquired its susceptibility, it certainly was a very unusual circumstance; for, as I have already observed, and as will be

* See Plate II. and case 10. in my work on Diseases of the Eye.

fully exemplified in the third case detailed, it in general requires a considerable time to restore by exercise the want of sensibility occasioned by its having been long dormant.

I have, however, lately seen another similar instance in a young man thirty years of age, born with cataracts: with one eye, as in the above case, he never had seen more than to distinguish brilliant colours, and perceive light from darkness. The fragments of the cataract having spontaneously become depressed immediately on its being divided, he instantly saw light much more strongly than he had done before; on the following day he saw all the objects around him, and at the expiration of a fortnight, when the eye had recovered itself, he could, with the assistance of glasses, distinctly discriminate the minutest objects, even the second and minute marks on my watch dial.

The third case is entirely opposite in its bearing to the two former. About seven years since, I operated upon a young gentleman born with partial cataracts. The centre of the crystalline lens was opake in both eyes, but the circumference in each was transparent, which afforded him an indistinct but very useful vision, he being able to read small Greek character, and to see distant objects, although not as far or as distinct as other people, yet sufficiently for general use. I removed the whole of the lens by the operation for absorption, when with the assistance of properly adjusted glasses, he saw with his best eye both near and distant objects to the utmost extent of his expectations and wishes; with the other, the retina of which had become insensible from never having been in the habit of exercising it, he could see but very little. After the operation he habituated himself constantly to use spectacles for distant as well as for near objects, and he is now at the end of seven years unable to avoid running against persons, or the furniture of a room, if he attempts to cross it without them; whereas, with spectacles, he sees sufficiently to have become an expert shotsman. There can be no doubt that the inability of this gentleman to see objects, has arisen from his not bringing the adjusting powers of the organ into action, in consequence of habitually using his two kinds of spectacles which effected the necessary refraction of the rays of light to see both near and distant objects, without occasioning any exertion of the eye; whereas if he had felt the same necessity of doing without them soon after the operation, as described in the case of the postillion, he would unquestionably have succeeded equally well. It is also evident that by the removal of the crystalline lens, he lost the power which he previously possessed, of adjusting his eyes to different distances, although their partial opacity prevented his seeing either near or distant objects as well as other persons, and which power, owing

to

to the habitual use of convex glasses, of $2\frac{1}{2}$ or $2\frac{1}{4}$ for near, and 4 or $4\frac{1}{2}$ for distant objects, he has not since reacquired.

From a consideration of these three cases, am I not then justified in hazarding the conclusion, that whatever power the crystalline lens possesses of adjusting the eye to different distances, yet, after its removal, there is another power of adjustment, which (as in the former cases) can be subsequently brought into action by exercising the organ without glasses, but which power (as in the latter) is not called into action when glasses are continually employed?—I have myself full confidence in this opinion, having, after the removal of the crystalline lens, almost uniformly witnessed similar results from similar exercise of the eye.

The partial insensibility of the retina before spoken of, as a cause for the necessity of using a deeper convex glass soon after the operation, for the purposes of vision, than is afterwards required, cannot apply to the best eye of this gentleman, for to the period of the operation he exercised it in the prosecution of his studies; while its necessity for the other, the retina of which had become torpid for want of being exercised, was strikingly exemplified, it being a considerable time after the operation, even with the assistance of the deep convex glass, before this eye had acquired any degree of useful vision. At the present time its powers are very inferior to the other, from being less employed in consequence of the patient's chiefly relying on his best eye.

In returning to the consideration of the case detailed of the young woman with the conical cornea; it may perhaps be supposed, that, by admitting the susceptibility of the retina to have been increased by its being twelve months exercised after the operation, and the adjusting powers of the eye to have been acquired from the same cause, I abandon my opinion that the *morbid* degree of refraction of the light in its passage through the thickened cornea, together with the *natural* refraction produced by the crystalline lens, were the cause of the confused and imperfect vision previously experienced by the patient: this however is not the case, as the fact of the girl being capable of seeing after the removal of the lens, *which was not in the slightest degree opake*, after having been blind previously, convinces me that the refractive powers (the conical cornea and crystalline) were too powerful, and that the cure was effected by the removal of one of them. But what I conceive proves the accuracy of these inductions is, that in the earlier stages of the disease, when the thickening has not attained the height it had reached in the case alluded to, the greatest assistance is afforded to the patient's vision by the employment of concave glasses.

It is not, however, my intention to urge that the refractive power is equally great in the thickened cornea as in the crystal-

 line

line lens; on the contrary, I think that a convex glass of a less magnifying power than that usually required after the removal of cataract, may be frequently employed to great advantage in cases of conical cornea.

Were any further arguments than those adduced necessary to prove that the short sight of the patient is occasioned by the morbid thickness of the cornea, and not by the superabundant quantity of the aqueous humour, as has been supposed, it would be, the well-known fact, that water possesses little comparative refractive power; while, from the dense structure and the form of the conical cornea, it is as evident that its powers of refraction must be very considerably increased. Indeed, *gutta serena* would certainly be produced by the backward pressure of a superabundant quantity of aqueous humour against the vitreous humour and retina, long before it would occasion a protrusion of so dense and firm a tunic as the transparent cornea; and I have actually seen *gutta serena* result from this cause, without any material convexity of the cornea being perceptible, although from over-distention it felt to the touch nearly as hard as an egg-shell.

Although I may have failed in convincing my readers of the accuracy of some of the opinions which I have ventured to submit in this paper, yet I have the gratification of knowing that I have fully proved the important fact, of having successfully carried into effect a mode of treatment capable of restoring vision in a case incurable by other surgical means, and which, as far as I have been able to ascertain, has hitherto never been employed by any other person.

XXVIII. *On the Phænomena of Platinum and other Wires in inflammable Media.*

To Mr. Tilloch.

Sir,—Admiring, as I did, the important and beautiful communication of Sir H. Davy on the Phænomena of Platinum Wire and that of Palladium in some inflammable Media, it may not be uninteresting for you to receive an account of my experiments. "*Experimentum fiat*" is the impression which will ever govern my philosophical studies: and seeing that the noblest erection of theory which the human mind can rear, must yield the precedency to a single fact established by the hand of experiment, I shall in the present imposing march of chemical discovery be fearful of trusting to the illusions of fancy, and regard with a jealous eye the portraiture drawn by the pencil of hypothetical dogma.

1. Plati-

1. Platinum wire continues red hot in explosive mixtures of inflammable gases. Also in the vapour of naftha, alcohol and ether. I found it in like manner effective in *nitric*, &c. ethers, as well as the sulphuric; in *phosphorized* ether and *alcoholic solution* of *camphor*; *oil of wine*, vapour of *sulphuret of carbon*, and of *camphor*.
2. I discovered that gold, silver, steel, and copper wires were *phosphorescent*, though they did not continue red hot in the vapour of *ethers*, *oil of wine*, and *sulphuret of carbon*.
3. *Platinum wire* and that of *gold*, *silver*, *steel*, and *copper*, when phosphorescent in the *vapour of ethers* and oil of wine, evolve a peculiar acrid acid gas, rendered sensible by the contiguity of a stopper moistened with ammonia. This acid gas acts violently on the eyes, exciting tears.
4. A *glass rod* and piece of the stem of a *tobacco-pipe* will, in the vapour of ethers and oil of wine, be phosphorescent; and in like manner contribute by this minimum combustion to the formation of the acid gas in question. I need scarcely add that, prior to the introduction of the wires, &c. into the volatile inflammable medium, such should be made *red hot*.

The phosphorescent flame is very livelily exhibited in introducing copper wire, &c. into phosphorized ether; and the platinum wire continues to glow very vividly over oil of wine, until the ethereal matter is all expended in the production of the acid gas, leaving a residuum, which seems to be a *peculiar fixed oil*, combining with potassa and soda, and highly inflammable. Spec. gr. less than that of water.

5. When gold wire entwines a coil of platinum wire, and the wires are heated and plunged into the ethereal medium, the platinum continues to glow red hot, and the *gold wire* will be seen *dark* among the threads of platinum.
6. The phosphorescent flame exhibited under the preceding circumstances I did not find susceptible of igniting either olefiant or coal gas, or phosphorized ether, sulphur, or sulphuret of carbon. A jet of coal gas urged on platinum wire red hot, in oil of wine, &c. caused it to glow more vividly. If the platinum wire be depressed lower than the focus, where the ignition is most intense, the phosphorescent flame will be displayed, and the platinum will cease to be red hot.—The display of phosphorescence may be repeated frequently, particularly with glass and alumina; for at each successive immersion it will be renewed. Two coils of platinum and steel wires in contact, both previously made red hot and plunged simultaneously into the vapour of ether, exhibit an interesting appearance. In the first instance the steel wire will continue

continue red hot, while the platinum is not luminous. On the moment, however, the reverse takes place, and the platinum becomes luminous, while the iron wire instantly ceases to be so; nor does the latter exhibit phosphorescence when the platinum glows.

I am very respectfully, sir,

Your obedient humble servant,

Surry Institution, Feb. 3, 1817. J. MURRAY.

P. S.—I plunged the red hot platinum wire into the vapour of the following essential oils, but did not succeed in maintaining it in this state—*Origanum, nutmeg, lavender* and *cloves*. In the *vapour of sulphur* I had a lambent phosphorescent flame.

XXIX. *Geological Queries, regarding the Coal Strata, Basaltes, and Red Marl, of Northumberland and Durham, and on the Appearances of Coal, &c. in Lincolnshire. By* A CORRESPONDENT.

To Mr. Tilloch.

SIR,—THE extreme kindness and urbanity of your very able correspondent *Nathaniel John Winch, Esq.* in replying, so promptly, in p. 100 of vol. xlvii, to the 5 queries which I had taken the liberty of proposing to him, in p. 12 of that volume, ought long ago to have received my grateful acknowledgements: but gathering from his replies, that the *Geological Society* were in possession of important communications from him, on the subjects of my inquiries, I have abstained from troubling him further; until now, when having read the 1st part of Vol. iv. of their Transactions, I am happy in being able, sincerely to thank and congratulate Mr. W. on having produced (*near 3 years ago*) beyond comparison the best Paper, for its copious and *practically useful details*, which that Society have yet laid before the public: accompanied at the same time by my regret, that the Society have not thought fit to include also in this part, another Paper, which I learn that Mr. W. has (more than 9 months ago) presented to them, regarding the east part of Yorkshire.

Mr. W.'s goodness, and zeal for the advancement of practically useful Geology, will I trust excuse the liberty I am taking, in again reviving, and considerably amplifying, some of the subjects of my Five Queries, in the page above referred to, viz.

1st. Are not some of the lower individual seams of *Coal*, and their floor and roof Measures, of Limestone, &c. which are worked in the detached part of Durham and NE part of Northumberland, traceable, although perhaps varying much in thickness and quality, round the east and south flanks of the Cheviot mass

mass of strata, with corresponding dips, somewhere near to Rothbury?, Bellingham?, and thence W and NW, towards or into Cumberland?.

Do not some of the Coal-seams, and their accompaniments, which are wrought near the Coast, between the Coquet and Wensbeck Rivers, range thence, varying perhaps in thickness, somewhere near to Morpeth?, Stamfordham?, Hexham?, Stublic? and Alston-Moor?; and do they not continue thence through Teesdale Forest?, to near Staindrop?, and become covered by the Magnetic Limestone?; And, do not these same measures, occupy part of the surface E and SE of Brough?, and in Arkengarth Forest?, &c.

2nd. Is not the *Basalt*, ranging from Jemming or Timming, (or Temon) to the Tyne River near Wall, and from West-Harle to near Causway-Park, in reality *a continuous stratum*, with a south-eastwardly dip?.

Are not the scattered patches of Basalt, between the Tyne and West-Harle, in reality, either joined, by connecting, but *thinner parts* of the same stratum, (and perhaps greatly *varied in composition and appearance* also)?; or, does the continuous bed of Basalt alluded to, range on the SE of most of these patches of Basalt, leaving them really detached, as Hummocks?.

Is there not a prolongation of the Basalt, traceable from Causway-Park eastward to the Coast?: at least, do none of its numerous varieties appear, *in situ* (not as real *dykes*) on the shore, or in the adjacent Islands, from Druridge to Red-Hill, near Long-Houghton?.

Are the apparently detached parts of a Basaltic stratum between Bamborough-Castle and Kyloe, really detached?, (not owing to alluvia or peat, making them appear so); and if so, do not the dips of the strata indicate, that such Hummocks occupy the lower parts of a *trough* in the measures?

In ascending the Tweed from its mouth, where does Basalt cross its bed? or appear in its banks?; or over-lieing Red Marl or its Sandstone commence, to prevent further observations on the regular or continuous strata of that district?.

Again. Does not the Basaltic stratum, at or to the W or SW of Jemming, assume, first a S and then a SW dip, so as, in reality, to range away NW?, instead of SW, to pass Richargill and join the eastmost of the two ranges of Basalt depicted in Mr. Professor Buckland's Map, in Vol. iv plate 5 of the Geol. Trans., as seems intimated near the top of p. 117?

Do not Mr. B.'s two ranges of Basalt belong to *the same stratum**, (however different its composition, thickness or the rapidity

* The *Querist* would feel exceedingly obliged to Mr. Winch (or to Mr. Frere,

dity of its *contrary* dips)?: and do not these two ranges of Basalt, in reality *join* and form a very lengthened and irregular ring? (or rings, if they join in more than two places?); if so, where are its ends situate?; and if not, in what way are the two ranges terminated or lost sight of, at one or both of their ends?

Does the Basalt of the upper part of the Tees (mentioned pp. 73 and 74), really basset there towards the S?, showing its upper-measures on the N, and its under-measures on the S of the Basaltic Area?, (and in such case, does it join, and by what route, to the easternmost of Mr. Buckland's ranges?; and can it be traced the other way under the magnesian Limestone?); or, does it dip and take cover on the S side, as well as the N?, appearing *to day*, in consequence of the upper or Lead-measures being excavated from off it?: in the latter case, is one or more rings formed, by the basalt also being locally cut through?, if so, what strata (besides Hornstone or Chert, p. 74) are thereby exposed to view?.

3rd. Are not the seven localities* of *muscle-like Shells*†, in the Ironstones and their accompanying Shales, which Mr. Winch

Frere, or any other Gentleman) for revising and freely stating the facts, as to the interesting district which Professor Buckland has described Geol. Trans. iv. p. 105, (and Mr. Joseph H. Frere, Phil. Mag. 47, p. 42): and particularly to say, whether the "Sandstone," left without an ascertained thickness, at the bottom of p. 62 in Mr. W.'s Paper, means the extended and irregularly thick stratum or mass of over-lieing (and in part unconformable) Red Marl and its Freestone and Gypsum, mentioned by Mr. Forster p. 43, and shown by Mr. Buckland *west* of both his ranges of Basalt?; or, is it the defined and comparatively thin stratum, coloured red, *between* the two Basalts, and apparently, conformable to the eastern range?—the average and extreme visible thickness of this stratum?: its roof and floor measure? &c.—because, "*imaginary*" Sections, and "*supposed* relative positions" (p. 118), do not satisfy the Querist, who has no knowledge, or desire to know, whether the epithets "*old*" or "*new*," should designate *either* of these strata.

* The Querist regrets vastly, the smallness of this number of precise localities:—to similar disappointments he has often been left, regarding phænomena elsewhere, said, in the loose and general way of conversation, to occur "in every Colliery," "in most places," "everywhere," "all over the district," "in every parish," &c. &c.:—all his researches into the localities of fossil Shells, particularly of muscle-like shells in the Coal districts, forbid however the supposition, of such belonging to *many*, or to slightly marked parts of the series of strata: he intreats therefore the kind attention of Mr. W. and others, resident in or acquainted with Northumberland and Durham, to multiply those localities, as much as they may be able: adding, whenever they can, a reference to the particular stratum in Mr. Winch's numerous sections, in which such Shells occur.

† Whether *marine* or *fresh-water*, he willingly leaves to theoretic speculators.

has

has mentioned, referable to two very different parts of the series? (separated by the Basaltic stratum, the subject of the last head of Queries): viz. 1st, those of Holy-Island (N shore?) and of Hairshaw, to the Craw-coal and Limestone series below the Basalt?; and, 2nd, those of Low-lights Rock at the Tyne mouth, of Merton (not Muston?), of Hebburn, of Heaton-dean, and of Wylam, to the thick Coal series (or some *one stratum* therein?) far above the Basalt?

4th. Does it anywhere appear on the surface, or has it been anywhere proved by sinking or boreing, that the Red Marl or its Sandstone, of the lower part of the vale of the Tees, is actually "*covered by* the alum shale" of the north-east part of Yorkshire?: and if so, at how many and what places?.

5th. Where are the ash-gray fragments of shelly bituminous Shale (P. M. xlvi. p. 466) most copiously washed on the shore, of the Lincolnshire Coast? The shelly bituminous Shale near Bolingbroke, certainly is not situated *immediately* below the hard chalk, as Mr. W. seems (P. M. xlvii. p. 101) to suppose: but from the Querist's observations, he is inclined to think, that the same may almost as deeply underlie the chalk of Walton, NNW of Wainfleet, as the Coals of Easingwold do the Chalk of Acklam, or Bishop-Wilton, N of Pocklington; or as deeply as the Coals of Danby Dale, E of Whitby, underlie the Chalk of Hunmanby, &c.

He believes, that neither the Amonites or other Shells† of the Danby Dale or Thirsk or Easingwold Coal series, will be found wanting in the Clunch Clay, in any part of the range which Mr. Smith has traced for it, on his Map of the strata of England.

Trusting to the kindness and liberality of Mr. Winch and others for resolving those Queries, with as much expedition as is consistent with circumstances, I remain

Jan. 7, 1817. A Constant Reader.

XXX. *On the Physiology of Vegetables.* *By Mrs.* Agnes Ibbetson.

To Mr. Tilloch.

Sir, — I now write to show that I am not the original author of the discoveries made and presented in my last letter; for I find, with the greatest joy, that it has been previously published and acknowledged by one of the cleverest men, and the most scientific observer, of the last century. Mr. Henry Baker is the person to whom I allude. Unfortunately he did not (I suppose for want

want of time) pursue the fact through a series of inquiries, as it should certainly have been done. Such discoveries have often been thrown off merely because they want the leading points to serve as *conductors*, or the *continuing line* to pursue the course begun. Possessing such a fact, so *strong*, so *important*, it should never have been neglected, but traced in its preceding and following points: if this had been done, the whole circle of vegetable life would now probably have been *well known* and understood.

I showed in my last letter, that seeds, when cut into extreme thin slices *horizontally*, gave the appearance of many very diminutive shoots, with a root to each, or a long continuance of a thin narrow pattern running all over the seed, and surrounding the embryo at a small distance. I was not the least aware that this was before known; since it had not at all *corrected* our ideas concerning the formation of plants, which supposes but one germ in each seed. But I now find, in looking over The Philosophical Transactions for the year 1739, that Mr. Henry Baker had in that work given an account of his discovery, which I here copy from his own letter.

"The growth of animals and vegetables seems to be nothing else but a gradual unfolding and expansion of their vessels, by a slow and expressive insinuation of fluids adapted to their diameters, until, being stretched to the utmost bounds allotted them by Providence at their first formation, they reach their state of perfection or full growth. If this be granted, the consequence must be, that all members of a perfect *animal* exist really in every *animalcula*, and that all the parts of a perfect plant exist in every little grain of the *farina plantarum, however minute.* Amongst numberless inquirers whom the opinion "that every seed includes many real plants," (has set to work to open all kinds of seeds and try by glasses to find evident proofs thereof,) I have not been the least industrious: but after repeated experiments in every manner I could think of, I began to despair of success. If by moistening the seed it began to vegetate, I could indeed discern the seminal leaves and the germen or bud whence the future plant should arise, but was able to go no further, unless I waited till the moisture gradually distending its vessels, made the little root or heart shoot down, and stalk rise up, and the minute leaves expand and bring themselves to view. This however was not the thing sought: but some days ago, by mere accident, when I thought nothing about it, I was favoured with a discovery I had so often searched after to no purpose. Endeavouring with a fine lancet to dissect a seed of the *Gramen tumulentum*, with intention to examine the several parts of the case with a microscope, I struck the point of the lancet into something

something exceedingly small between the two sides in the contexture of the husks, the edges of which I observed to be transparent. I opened them lengthwise, so as to possess myself of a very thin piece which set at liberty my object, which on examination proved to be a shoot with a little root to it, seen at fig. 1, (Plate II.) *Plate* No. 5. After viewing many parts of the husk, as it *really was*, it appeared to include many perfect plants. This was a sight I so little expected, that I distrusted my own eyes, and examined every way I could contrive, to prevent being deceived; but in all *lights* and in all *positions* it presented the same figure. I afterwards cut open a great many different seeds, and found in all the same number of germs. I endeavoured to separate a few of these minute plants from the *theca**, and succeeded, (see fig. 2.) I thought this account so novel and curious, it might be acceptable to the Philosophical Society, to which I now send it with the drawings, and a drawing (fig. 3) of the part of the seed surrounding the embryo, and the shoots which appear in it.—*Henry Baker.*"

It will be thought perhaps extraordinary that I should be so delighted to find this part of my discovery *forestalled*. But first, I am truly charmed to get *any additional* proof of the truth of a part of my system: and I am really so anxious to establish the foundation of *botanical knowledge*, and ascertain the first principles of the science *on a sure and solid ground*, and facts that the eyes as well as understanding will vouch for, instead of being, as it is now, a parcel of detached imaginations, so contradictory in themselves that not three botanists think alike upon any one subject, and most of them are contradicted by Nature and dissection,— that I care very little who makes the discovery, provided it is just and true, and will contribute to advance the science: and it is adding a proof, to what I have already shown respecting the various shoots in the seed, displayed in my last letter, that makes it really a most *precious piece to me*. The next thing therefore that occurred to me, was to try whether these germs (found in the case of the seed) *would increase* if water was put to them while on glass. This they did in a few days; which proves also that I was right in the trial I made of them as stated *in my last letter*.

In my last, I also showed that, wishing to prove that those balls in the root were truly the *heart of seeds*, (as I have before shown by every other means possible,) while still in the radicle; I shook them out on glass, covered them with a drop of water, but just enough to wet; when the *seed* (which was before so dimi-

* "*Theca*," an excellent name for the sort of powder that fills up the spaces between the different shoots with their roots. I shall call it so in future.

nutive

nutive as to be hardly visible) began in a few hours to swell, and in two days the shoot had run from the heart of the seed; but it was very different from the shoot which leaves the full-grown seed: it was evident that this was not yet *formed*, that it was merely the *heart* of the future germ, which contained the little shoot of leaves and female flowers of *that female*, which passes from one seed to another, from one plant to another, which is sent *up the radicle* as a beginning for the *new coagulated seeds*, just formed from powder, and which serves (I think) not each for a new plant, but for the foundation of each different stripe of flowers which runs up the wood. So direct does this fact appear to me, that I think it almost impossible to doubt it. To me indeed, who had seen the flower coagulate in the end of the radicle, then form into balls and pass through the root, enter and run up the alburnum, pass into the flower-bud, where in a short time it was fructified and its case added—it was impossible, after such a series of proofs, to doubt that these balls were seeds; but by botanists in general, I am told, stronger proofs were demanded: but the growth of the shoot in the heart of the seed must certainly be *perfectly convincing*. What more can be required!

Mr. Baker adds these lines:

"Each seed includes some plants, those plants again
Have other seeds, which other plants contain;
Those other plants have all their seeds, and those
More plants again successively inclose.
Thus every single berry which we find
Has really in itself whole forests of its kind."

Now this appears to me to be carrying the evidence far beyond what the appearance will justify. Mr. Baker had made no preceding dissections that could trace these shoots; and therefore looked on these diminutive roots found in the husk of the seed, as each a separate plant, or new germ. But I, having before traced them in *every part of the vegetable*, cannot think as he did in this respect: they appear to me to be only the means Nature takes to prolong the female flower, from root to root, from seed to seed; and that the seed, with its different roots and its embryo, serves only as the different parts of one plant, and no more; and I think the following specimen, which I have dissected, of a bean will justify my opinion, (see fig. 3, cut open.) Here the seed *aa* is retained close to the plant, though the embryo has left it, till the vegetable has enlarged its wood and line of life sufficiently to take in the female flowers which pass through the two *holders bb*, of *the seed aa*, and run in stripes into the wood. I give, however, the specimens as exactly as I found them: Let every one explain them as they please.

please. But these roots do not appear to me to be *separate plants;* but only the foundation of the *various stripes* in the wood, and the means Nature takes to secure the *impossibility* of the female ever failing. Had they been *separate plants*, and to serve as *new seeds* (as he supposes), would they not have fallen into the earth to serve as such, and not attached themselves to the new plant a *second time*, though the embryo had already escaped from it? Thus the husk is secured, not only to supply new moisture, to form a new root in case the young should be destroyed by insects; but that the different little roots might have time to run their stripes up the various parts of the wood, and thus fill the new plant with the female flower.

I am assured by some of the first botanists, that they dispute not the truth of my dissections—that they believe them all to be *just* and *right;* but that it is my *system* they cannot credit. I know not how this can be: I always thought that what is *so called*, was but my drawings *translated into words*. However, I dispute not the right each person has to explain the specimens in their own way. This last will certainly admit of two different constructions; and by continuing the dissection of plants a year or two longer, we may be more fitted to lay down a system that may be nearer the truth than that I have attempted: yet, having followed it from year to year in a regular picture, a few days only intervening, *no one can conceive* (but myself) *how exact such a picture is*. But one thing I must observe, that if the drawings *are true*, the very opposite of the specimens cannot *also* be just; and that most of the present received opinions are in direct contradiction not only to Nature's established laws in the physical and botanical world, but they are also in absolute contrast to the appearance of specimens I have given, and even to their *own also*, as I shall show.

I shall first present *two specimens of wood*, given by most botanists, to prove that there is *no passage for the sap through the wood*, as was always formerly supposed. The one is a specimen given by Grew, the other by Mirbel. It would appear by a first look, that large apertures fitted for the nourishment of a *great tree* were plainly to be seen in both (see *dddd*, fig. 5 and 6,); but our later botanists are of a *different opinion*, and throw all the juice so *large a matter would seem to require, into the smallest hair the tree contains*, and which is besides *most extremely twisted*. The reason given for it is, "that though there are apparent large apertures, and well formed voids, yet, when the wood is taken thread from thread, not a *cylinder* can be discovered with a *passage through* it. This is *certainly true;* hence it is concluded, that, *in spite* of the *appearance* of *these apertures*, *there can be no passage* for liquids through the wood but

but the *diminutive spirals* before mentioned. Yet surely this conclusion is premature, and does not follow the *premises.* If a solid matter is taken, and a quantity of *large holes drilled* through it, in its *whole length;*—if that matter is capable of being taken to pieces *thread* from *thread*, no *vessels* will appear—still no one will deny that in its perfect state it was capable of receiving a liquid, and passing it through these *drilled passages!* Such is the formation of *the wood*: still it may be *torn* and separated into the most *minute threads*, though certainly capable of receiving the sap in the many apertures and long *voids* formed in it for the purpose. And to prove that it does so, and that *voids* instead of *vessels* are made for that purpose, a person need but place a piece of wood in a coloured liquor, and it will taint each vacancy throughout its whole length, in a manner that must plainly prove that the coloured liquor runs completely through them. Or, if quicksilver (with the help of an air-pump) is passed through the wood, it marks with black streaks each projection of the *voids:*—nay, if a specimen is cut extremely thin, and placed in the solar microscope, it will still more evidently show this, by the sap running up the *very passages* before *your eyes;* and, if quite a fresh piece, will continue to move thus for near *an hour.* The passages cannot be mistaken, because they may be strongly marked before they are put within the glass. Nor is it difficult to understand WHY *Nature* has made *voids* in the wood instead of VESSELS. As the flower-buds pass *across the wood* when first running to the bark, if they had been *vessels* they would continually have been twisted and impeded in their progress, stopping the liquor they contained; but, formed as a *void*, they are so large as to bear being pushed a little out of their places, yet not impeding the liquid (see fig. 8, *gg*, buds); and being surrounded every half or quarter of an inch at the top with the spiral wire or muscle of the plant (see fig. 7, *ff*) the wood is soon again (by its own contractions) restored to its usual place and parallel circle. No person can cut and place a piece of the wood in the solar microscope without seeing this, and without understanding the whole process. All the French botanists believe as I do; that these *voids* are made for the sap, and that the spiral wires have no passage for liquids. Mirbel, one of the best botanists of France, says: "*La trachée végétale est un tube formé en spirale par un filet tourné de droit à gauche: ce filet est *opaque, brillant, argenté,* épais: sa coupe

* The spiral vessels are tubes which wind round a centre from right to left: they are perfectly opake; and when cut across, a flat figure or ellipse is discovered, brilliant, silvery: sometimes three threads are retained by a cross membrane; but I never could perceive (says Mirbel) any opening to them, in spite of the idea of some authors who are of this opinion.

transversale

transversale est une *lame* plate qui m'a présenté une *ellipse*, ou quelquefois trois filets réunis par une membrane; jamais je n'ai peu appercevoir l'ouverture d'un tube, comme plusieurs auteurs l'ont avancée."

If there is no passage in these spiral vessels, they cannot carry the sap; and it must pass through the vessels, or rather voids, which so plainly appear in the wood at fig. 5 and 6. Indeed the French authors are all of this opinion; and of *dissectors* I know but Mr. Knight, who turns over the office to these diminutive twisted vessels. Du Hamel looked on the spirals as containing air only: but they have a much more important office, and are certainly the universal cause of *motion to plants*, resembling the animal muscles in every respect, and possessing, like *them*, that *vis insita* which is a proof of their identity *impossible to deny*.

If, therefore, the account given of the figure of plants do not agree in the interior, or answer to their appearance, when given by my opponents, though adduced by them as exact,—brought forward by themselves; How should my *drawings* agree with *their accounts?* when their accounts do not agree with *their own drawings*; for as a void will do as well as a vessel, the reason against the *flow of the sap* in them vanishes. And as the wreaths of flowers display themselves coming out of the seed in December, before the seed has passed up the stem, those flowers cannot be formed in the flower-bud the following May. And as the seeds which contain these shoots appear in quantities in November, and can be made to increase their premature shoots before they rise in the stem; they cannot be produced in the following July and August in the seed-vessel at the top of the plants.

I am, sir,

Your obliged servant,

Exeter, Nov. 2, 1816. AGNES IBBETSON.

Description of the Plate No.5, [see Pl. II.]

Fig. 1. The shoot mentioned by Mr. Baker.

Fig. 2. The exterior of the seed or husks.

Fig. 3. The figure of the embryo of the bean, with the seed *a a* joined to it.

Fig. 4. Piece of the wood as I see it with the flowers at *m m*.

Fig. 5. Piece of the wood given by Mirbel, without the flowers.

Fig. 6. The wood cut horizontally, given by Grew: sap-vessels *d d d*.

Fig. 7. The sap-vessels, showing the spiral at the top which contracts and stretches alternately to let the buds pass.

Fig. 8, showing the manner the wood changes its figure to let the buds pass.

Fig. 9. A piece of the root where the seeds cross it to gain the alburnum vessel.

Fig. 10. *hh* the young shoot growing out of the seed.

Fig. 11. Manner in which each separate seed grows in the glass when they are thrown on it. This perhaps makes one of the most beautiful specimens imaginable, and is varied in each different plant. In cutting the root of the chickweed in October, I flung the seeds out of the root on the glass. This is not so easily done as in a bulb; but with the help of a diminutive bit of the interior I *succeeded*, and a more beautiful picture of the sort of the stem which proceeded from each seed is impossible to express; even the very bulb of the stem was visible, (see fig. 1, 2) from each of which proceeded a quantity of most diminutive leaves, with the usual bladder or flower. I am particularly careful when I mention the flower, to show that it is merely a little bubble with a mark within it, as thus,  or the pistil and corolla alone.

XXXI. *On Astro-atmospherical Science. By the Rev.* T. Drummond.

To Mr. Tilloch.

Sir, — The truth of astro-atmospherical science is not determinable by plausibility of argument,—the only test by which its foundation in Nature or its baseless fabric in visionary hypothesis can be ascertained, is that of experience. By advocating this subject, I do not intend to throw down the gauntlet in defence of all that astrologers, ancient or modern, may have advanced; and I consider your admission of my papers as no proof of your coincidence in opinion, but as a laudable proof of your liberality in the investigation of a doctrine which was maintained in ancient time, has still numerous advocates in Eastern countries, and which, *if true*, will again in a *series of revolutions* be generally admitted.

In our modern works on the sciences no traces of the elements of a study of sidereal influences are retained; the subject is mentioned as a chimerical hypothesis; and it requires a hardihood of daring, or a persuasion of the truth, to advocate what so many wits are ready to ridicule, and so many learned men are forward to speak of in terms of *contempt*, *reproach*, and *reprobation*.

I could supply your pages with a multitude of plausible objections to this science: but that is not my province in this correspondence;

respondence; and I frankly acknowledge myself to be incompetent to reply to all the objections which may be offered. As on every other subject, so on this, I hold myself ready to embrace conviction, whether substantiating or overthrowing a hypothesis which I now believe to be founded on the established laws of Nature, although long observation and accurate deduction may be requisite to recover the knowledge which has been lost.

I cannot apprehend that any of your readers will take *alarm* at an attempt to obtain for the doctrine of sidereal influences on the atmosphere a fair investigation. The *ignorant* might suspect the subject of atmospherical astrology to be somewhat allied to MAGIC or NECROMANCY—the probability of such an error I infer from having some years ago experienced the consequences of ignorance respecting those imaginary phantoms imputed to diabolical agency.

A young man bathing at Ellingham in Norfolk was drowned. A *female Samaritan*, possessed of more information than her neighbours, employed the means recommended by the Humane Society;—but, amidst the *ridicule* of the *ignorant* and the *terror* of the *superstitious* at her presumptuous attempt to animate the *dead*, just as some signs of resuscitation appeared, sunk under her fatigue, and the body became a corpse past all recovery. I rode through the parish before the villagers had dispersed; but some hours having elapsed, I merely joined in the conversation of the groups through which I passed, to the confusion of the incredulous, and in corroboration of the opinions of a few who had arrived too late to contribute their willing aid. About the same time I was trying some electrical experiments on a young fowl which I found as I supposed *dead* in the yard more than an hour before. My object was to notice the most ready passage for the electric fluid. During the experiments I was surprised by some signs of returning life, and persevered with different motives to those at my commencement. Having applied many small shocks through the brain, and numerous sparks in various directions, I placed it in a jar which I continued to supply with a portion of electric fluid insufficient to produce a spontaneous explosion. I continued my varied processes about three quarters of an hour, by which time it was so far recovered that weakness was the only indication that it had ever ceased to manifest all the signs of life. I electrified it at liberty the next morning, when it fled from the sparks with as much agility as any of the brood. The fame of this occurrence was spread through a rustic neighbourhood, amongst whom there were many not *deficient* in *ignorance*; and so foolish was the idea entertained by them, that, when a child a few days after fell into a small body of water, my

 readily-

readily-offered assistance was refused with HORROR, because nothing less than *diabolical cooperation* could have induced me to expect to recover a life which God had permitted to cease.

I trust my motive in the insertion of these anecdotes will be obvious, as they serve to prove the vague ideas which have probably given rise to the old stories of *magic* and *necromancy*.

The experiments in the practice of what is now usually comprehended under Natural Philosophy, have ever appeared to the uninformed wonderful effects beyond the ordinary power of human nature. Hence FRIAR BACON and other *Franciscans* endured the censure of their ignorant contemporaries: and necromancy has, I conceive, no other origin than what has been ascribed to it by weak minds, whose terrors may have been augmented by the artful for sinister purposes.

Every friend of literature must regret the destruction of the library founded by Ptolemy Philadelphus, which is said to have contained four hundred thousand valuable manuscripts, when Julius Cæsar (47 A. C.) set fire to Alexandria.

A second library, founded by the successors of Ptolemy, intended to repair the loss of the former, and said to have consisted of seven hundred thousand volumes, was destroyed by Omar the caliph of the Saracens, A. D. 642.

Without noticing other collections of ancient literature which have been destroyed by conflagration, I cannot hesitate to express *regret* that the mathematicians were so indiscriminately banished from ancient Rome: nor can I see aught to commend in the voluntary destruction of books on the subject of natural philosophy, by some of the early converts to Christianity.—In those days all the labours of the learned were in manuscript; and we may infer that in numerous instances they were irrecoverable:—the impetuosity of the Roman warriors, the bigotry of the Saracen army, and the unrestrained zeal of the Christian converts, contributed to annihilate many monuments of ancient knowledge, whence we might have obtained the elements and the rationale of the systems which had in the most ancient periods of the world been maintained.

If, instead of such infatuated measures, we could now bring to the test of enlightened science, whatever contributed to raise the fame of those reputed learned, we might discriminate truth from error.

If (to descend to a later period), instead of acts of parliament against *witchcraft*, (which were not till within our own time repealed,) the development of the pretended secrets had been encouraged, the absurdity of the belief would have been most effectually exploded, and posterity would have been rescued from the possibility of imagining that nothing short of diabolical agency

agency rendered the united efforts of ecclesiastical and civil power necessary to counteract and suppress it.

If, instead of hearing that works of alchemy, &c. had been without examination destroyed, we, in this enlightened age, possessed an opportunity of exploring all that is comprehended under the denomination of *occult sciences*, we should be able to separate the chaff from the wheat, and demonstrate the means by which the fallaciousness of misconception, misinterpretation, or delusive artifice, had subjugated the minds of men.

The progress which has in late years been made in Oriental literature, encourages an expectation that the *arrow-headed character* and the *symbolic representations* still found on the remaining monuments of remote ages, may yet be deciphered, and serve to develop many particulars relative to the knowledge of the ancients in arts and sciences.

Josephus says of the sons of Seth: "These were the first that made their observations upon the *motions* of the heavens, the *courses* and *influences* of the stars: and having been foretold by Adam of a universal deluge and conflagration to come, they erected two pillars, one of brick and the other of stone, which they were sure would be a proof, one or the other of them, against either fire or water. Upon these pillars they engraved the memorials of their discoveries and inventions, there to remain for the benefit of ages to come; and lest the tradition of the science itself should be lost for want of a record."

Thence, sir, we perceive that this mode of perpetuating information is of great antiquity; and we are led to infer that the figures which still abound on ancient edifices, are not *mere ornaments*, but the CHRONICLES of *history* and *philosophy*, executed on a presumption that a *key* to their *symbols*, their *hieroglyphics* and their *characters*, would be preserved.

As the ruins of Persepolis and other temples indicate, by their partially mouldered columns and mutilated works of sculpture, the grandeur of design in those who planned, and the admirable skill of the artists who executed those edifices; it may rationally be inferred, that those adages of ancient philosophers which have escaped the ravages of time, afford us evidence that the premises whence their deductions were made, must have been the result of long experience and solid judgement, founded on the observations of natural philosophy aided by minute astronomical and mathematical calculations.—But to return to

ASTRO-METEOROLOGY.

An infallible scale of the weather has not been produced in modern times, nor is it to be expected without the concurrent testimonies of philosophers during a series of years.

None of the fragments of this science which have fallen under my observation are sufficiently minute to encourage a confident anticipation of all the phænomena which the changes in the atmosphere produce: and I conceive that an unreserved communication of conjectures,—those which have been verified and those not verified,—will be the most certain mode of putting to the test this almost exploded science.

With this view an anonymous paragraph was inserted in The Norwich Mercury, early in January 1816 (the weather being remarkably fine), announcing snow on the 28th or 29th of that month.

During more than twenty years I had observed ☌ □ or ☍ of ♄ and ☿ had been coincident with snow in the winter season, and rain in the other parts of the year. ♄ and ☿ were to be in conjunction on the 25th, but the moon was to be in conjunction with Saturn on the 29th. Some snow and sleet fell on the 25th at Norwich, latitude 52° 44′, and on the following days; but the greatest descent was on the 29th.

I think I may venture to assert, that whatever the result of the situations of the planets, according to ancient or modern anticipations, the effects are not fully manifested until the moon by her position becomes a powerful agent.

In Februrary, March and April, whenever the ☽ and ♄ were in any of the above positions, snow (more or less) invariably fell.

May 9th, □ ♄ ☿ melting snow or rain was to be expected, but the principal effect was on the 11th, when □ ☽ ♄ — ☍ ☽ ☿ and ☍ ☽ ☉ occurred.

Whilst delivering a lecture on meteorology on June 4th, I was requested to select some days, and offer my conjectures. On a hasty reference to White's Ephemeris the following were selected, having regard to the positions of the planets at and near the times. For an almost immediate reply to an unexpected request, no more than a cursory view could be taken. I have now subjoined the premises on which the conjectures were founded.

June 12th. □ ♃ ♂ — △ ☉ ♄ — □ ☽ ♃ — ☍ ☽ ♂; change from the present cold to warm weather; *perhaps* thunder.

July 18th. ☍ ♄ ♂ — □ ☉ ♃ — □ ☽ ☉ — ☍ ☽ ♃. Thunder-storm to be expected.

August 15th. ☍ ♄ ☿ — □ ☽ ☉ — □ ☽ ♄ — □ ☽ ☿. A heavy fall of rain.

Nov. 25th. □ ♄ ☿ — ☌ ☽ ♄ — □ ☽ ♃ — □ ☽ ♂ — □ ☽ ☿. Snow.

June 12th verified. Thunder within a few miles of Norwich.

July 18th. A violent thunder-storm at Newmarket; a house stricken

stricken at Ipswich (Suffolk) and a cottage within half a quarter of a mile of Norwich.

On the 4th of June the Bologna prediction was not known here; but I apprehend that the prediction of the *end of the world* was a mistaken deduction from the same premises as the above.

Aug. 15th. Verified.

Anxious to make another trial of the veracity of such observations, I affixed to the door of the corn-market an anonymous paper, September 21st.

Sept. 24th. Cloudy, with indications of rain; partial showers. 25th, 26th, 27th, fair; 28th, thunder clouds; 30th, fair in the first part.

Oct. 1st, RAINY WEATHER; from Wednesday noon, Oct. 2d, to the end of the week, fine and warm; Oct. 4th, thunder clouds.

N. B. November will be more gloomy and productive of sleet and snow, especially *towards* the latter end, than the month of December.

The above were verified, excepting that 2d, 3d, and 4th of October were not entirely free from partial showers.

On reference to your Magazine, it appears that

June 12. in the Journal kept at Boston, is recorded "*very fine.*"
——— Mr. Cary's Meteorological Table .. "*fair.*"
Oct. 1. Walthamstow .. .. .. "*very rainy.*"
——— Boston .. .. "*heavy rain till evening.*"
——— Mr. Cary .. .. "*stormy.*"

With due deference to the gentlemen who made the above observations, the selection of a particular hour does not admit of a register so ample as appears desirable. I have quoted the above, as they serve to corroborate the coincidences to which I wish to invite the attention of meteorologists, and propose hazarding some anticipations in a future paper.

The unusual prevalence of the north wind has been remarkable, and every fall of snow or cold rain has been accompanied with a north wind. Previously to an approaching agency of the ☽ and ♄, the clouds principally of the description small *cumuli* were to be seen posting with alacrity towards the north, impelled sometimes with a south-east and sometimes with a south-west influence; but on the days when the influences were to be expected most powerful, the wind, excepting in two instances (to be otherwise accounted for), became north

I will not trespass further on your attention at present, than to subjoin that November 25th, much rain fell in Norwich; but within about twenty miles, at Yarmouth, &c. a considerable fall of snow.

Much

Much remains to be ascertained in relation to different latitudes, and the effects produced in different parts of the day. I am unwilling to attempt to crowd your pages with too many of my own remarks, and hope that some of your correspondents will favour us with their observations.

Yours respectfully,

T. DRUMMOND.

Gray Friars Priory, Norwich,
Jan. 9, 1817.

XXXII. *Brief Notice of a Discovery by which Sir* H. DAVY'S *Safe-lamp may be made to re-light itself when extinguished in the Mine.* *By* J. MURRAY, *Esq.*

To Mr. Tilloch.

SIR, — IT gives me much pleasure to announce to you the following important fact:—a method by which Sir Humphry Davy's safe-lamp may be made to RELIGHT ITSELF when extinguished by an inflammable medium. The experiment was repeated eight times with uniform and unerring success in the laboratory of this Institution, on the evening of the third instant, before a number of gentlemen; and it was also proved that, independent of such a provision, the lamp must have been extinguished—a fact, indeed, that even the tyro of chemistry is well aware of; namely, the extinction of an ignited body when enveloped by an inflammable medium. For this purpose a metallic wire (platinum, steel, and copper were the metals employed) coiled up in a spiral form was exposed to the flame, close to the wick: the instrument was lighted, and, after some time, the wire, having become intensely red hot, was plunged into a vessel containing coal gas. At each sudden immersion the lamp was extinguished; but on the expenditure of the gas, instantaneously rekindled.

On the fourth instant, a number of similar experiments were repeated, and with results equally satisfactory. Different methods of keeping the wire in the flame were tried in these experiments. That represented in fig. A (Plate II.) was found to be a very convenient arrangement. The wire was from 1-25th to 1-30th of an inch in diameter; the diameter of the spiral about 1-3d of an inch; the number of revolutions in the spiral coil 7, and these about 1-30th of an inch apart.

When the lamp is kept well trimmed and supplied with spermaceti oil, the wire will be preserved of a temperature sufficiently exalted to rekindle the lamp on its extinction, so that all the

the miner will now have to do, is simply to depress the lamp into the lower strata of the atmosphere of the mine, and it will relight itself; and when exploring an extensive magazine of fire-damp, platinum wire, on Sir H. Davy's late interesting discovery, will be the preferable metal. The miner will not have to complain of "*being long in the dark.*"

This instrument is now one of the most curious and interesting that can possibly be contemplated—an envelope, *open*, yet *impervious to flame*—possessing within itself when *extinguished*, the property of exhibiting a *light unconnected with flame*, sufficient to guide the miner through the dark abysses of the earth, in an inflammable and explosive atmosphere; and the means of *relighting itself* on passing from an inflammable medium, which exceeds the explosive, into the free atmosphere. This mode of applying the platinum wire, you will at once perceive, is more important than suspending it in the top of the instrument, and it will not only not *intercept* light, but exhibit a *more brilliant* flame.

I am respectfully, sir,
Yours most obediently,

London, Surry Institution, Feb. 5, 1817. J. MURRAY.

P. S.—I rejoice in the near prospect I have of proving this in the mine itself. J. M.

XXXIII. *Reply to Mr.* HORN. *By Mr.* W. PATER.

To Mr. Tilloch.

SIR, — IN the last number of your Philosophical Magazine, Mr. Andrew Horn has condescended to notice some remarks I made upon his Theory of Vision in a former number; and with that suavity of manners and in that free and easy way so peculiarly his own, without a preamble he charges me with *ignorance* and *inconsistency;* and I think myself obliged to him that he confined himself to merely making the charges without attempting to prove them: perhaps it would have been better had he given the proofs and spared the insult.

In his answer to my remarks there is one trifling oversight;—he has totally forgotten to explain the difficulties, or to reply to the objections!

There is also a little unfairness which I have to complain of, which in Mr. Horn is perhaps not strictly honourable: in my attack I made use of *reason,* but he has defended himself with *insolence.*

Mr. Horn allows that light excites sensation by acting upon the

the optic nerve;—but for what purpose it is reflected back to the middle of the vitreous humour; how it is, and why it is, rendered caustic; why it is exhibited there, if it is not to be seen, and what is to see it, if it be not to be understood to be the optic nerve,—he does not attempt to explain.

Mr. Andrew Horn seems, somehow or other, now to be a little ashamed of his newly-discovered idea, that this image painted in the middle of the vitreous humour in caustic, is to be seen by the optic nerve behind; but the words which follow—"and thus the optic impression and position of the tangible object are reconciled,"—if they mean any thing, can have no other interpretation; for, to suppose that light is reflected from the optic nerve to form an image in the middle of the vitreous humour, and that then it is to be reflected back again to the optic nerve, merely for the sake of being sent there and back again, would be sending the light on what some would call a sleeveless errand, but I would call it a *Wycombe hoax*.

Instead of explaining these difficulties, Mr. Andrew Horn passes them unnoticed, and begins an harangue about Sir Isaac Newton's ignorance and his own correcter knowledge, and brings forward a few legerdemain tricks with a prism. Why this? I was not inquiring about Sir I. Newton's ignorance of light and colours, nor about reds, nor blues, nor holes in window-shutters! Why then were these things introduced, if not with the design of drawing the attention from the subject in question, which it did not suit Mr. Horn to reply to, for reasons which perhaps it is not difficult to guess?

However, upon closer inspection, this theory which Mr. Andrew Horn has given of the prismatic spectrum, has some little excuse for its intrusion, as it is evidently own brother by the father's side to caustic reflection, and appears to me to be equally whimsical, unfounded, and unsatisfactory!

Of this theory the hole of the window-shutter is the groundwork or first mover, its edge repels polarized light, which by that means is dispersed; which dispersed rays the prism collects, mixes together, and makes colours of them. Unfortunately for the theory, neither the hole nor the window-shutter is necessary; as the direct parallel rays of the sun, falling upon a side of a prism at a certain angle in *open day*, will be formed into the coloured spectrum, without any crossing of rays, inversion of images, holes, or window-shutters, having any thing to do in the matter.

But as I have no wish to hold any controversy with a man who arrogates to himself the privilege of insulting those who happen not to admit of his caustic reflections, without being convinced of the propriety of them, I shall make no further inquiries;

quiries: for, caustic as his applications are, I find them much more likely to *inflame* than to *cure* my mind of its tendency to consider his theory as absurd.

I am, sir,
Your most obedient servant,

Skeffhaven, Feb. 10, 1817. W. PATER.

XXXIV. *On Mr.* HORN'S *Theory of Vision.* *By* A CORRESPONDENT.

To Mr. Tilloch.

SIR, — THE perusal of a paper in your last, from Mr. Horn, upon Vision, induced me by its novelty to refer to his former communication, but without receiving any kind of satisfaction. His theory appeared to me, *primâ facie*, absurd, which I think a very little reflection will prove. He asserts that "light acts upon the optic nerve and excites sensation there, and not upon the retina; that the chief function of vision is assignable to the base of the nerve, and the optic images are formed by caustic reflection in the vitreous humour." I really do not understand his first assertion, that "light acts upon the nerve and not upon the retina," &c. if the retina is, as I have always understood it to be, a delicate membrane formed solely by the expansion of the optic nerve.

How is it possible that light, which is its proper stimulant, should excite sensation in the trunk, and not in the extremities of the nerve? Reasoning from analogy, we should infer the contrary to be the case. The other nerves of the body receive sensation only at their extremities. The depth in which their trunks lie buried, and an every day's occurrence after the loss of a limb, prove that, if the trunk of the nerve, which before the amputation of the limb had ramified upon the toes, be injured, the sensorium refers the pain to its former situation, and the patient does not feel pain at the injured spot, but in that spot where the sensation of the nerve had been usually excited; namely, in his toes. Besides, if the retina does not receive sensation, but is a mere reflecting surface, where is the use of its being composed solely of nerve? A membrane less liable to injury would have answered the purpose much better. The other proposition is, That "the optic images are formed by caustic reflection in the vitreous humour." I deny that the retina does reflect, or very slightly at most; the light of a candle is easily distinguished through it, which I imagine would not be the case if it was reflected. Allowing however, for the sake of argument, that it does

does reflect,—what is the use of the *pigmentum nigrum?* and how is the image formed by "caustic reflection" to be impressed upon the "base of the nerve?" It is not a tangible body: and Mr. Horn will not, I presume, have the boldness to assert that rays passing through a medium shall be reflected at a certain point, without any alteration in the density of that medium. Yet were all his positions correct, the vitreous humour must diminish the image, which will be considerably larger than the base of the nerve, and reflect it afterwards—a power which no one in his senses will allow it. What then would be the consequence of such reflection? Why, the rays would decussate each other in every possible angle; and instead of one we should have a hundred images, not one of which would approach the base of the nerve.

If these statements are true,—and I don't see how they can be disproved, as they are little more than a collection of facts,—What foundation has Mr. Horn for his theory?

Yours respectfully,

London, Feb. 18, 1817. J. Q. R.

XXXV. *On the Sulphuret of Carbon and on Flame, &c.* By J. Murray, *Esq.*

To Mr. Tilloch.

Sir,—In an early number of the "Annals of Philosophy," I pointed out some phænomena attendant on the great reduction of temperature by the evaporation of *sulphuret of carbon.* I find that if a few drops of this fluid be passed into a vessel of oxygen, it will *explode violently* on application of an ignited taper to the orifice. Sulphuret of carbon burns, when previously ignited in a medium of chlorine, but does not inflame spontaneously therein; when burning in contact with the atmosphere it exhibits an exaltation of temperature exceeding in intensity that of any other unexplosive flame, I have yet examined. *Steel wire* of 1-30th inch diameter burnt almost as vividly in this flame as in oxygen. Even a *watch-spring* fused with scintillation, and fine fibres of amianthus and platinum wire readily melted. Of the flames exhibited by combined hydrogen, those of hydrozincic gas and of boretted hydrogen appear to be the most intense; for in these, steel wire fuses vividly, and fine fibres of amianthus melt.

The blueish flames of alcohol, ether, &c. exhibit a *rose*-colour by the introduction of metals, charcoal, &c. and in the green flame of boretted hydrogen, &c. evolve a *yellow* colour. Scarlet, rose, and carmine, are the colours most generally produced with this

this treatment of blue flame, and various shades of yellow, &c. with green flame. I shall only observe in the meantime, I think it very probable that the *colour* of flame, and particularly of the *lamina* or border (which in that of sulphuret of carbon is a fine blue) with its (the edge) *width*, will determine the *grade of ignition*; and it is my opinion that all flames have a decided *electrical* character, which will express their relation to the intensities of ignition.

Oxygen compressed in Newman's blowpipe and passed through the *flame of sulphuret of carbon* produces a much more *violent* heat, than when alcohol is substituted as in Dr. Marcet's experiment.

I have lately made but few experiments with the compressed gases in the blowpipe. I however repeated successfully Dr. Clarke's experiment with the *rubies*; I also fused TWO SAPPHIRES into ONE. In one instance I completely *perforated* the *stem of a tobacco-pipe*, which the jet of inflamed gases *vitrified* in its passage,—a steel *file* before it, literally, as Dr. Clarke expresses it, presents "a shower of fire." In this experiment it is adviseable to detach the bladder containing the mixed gases, lest the sparks should ignite the explosive volume.

I am respectfully, sir,
Your humble servant,

Surry Institution, Feb. 20, 1817. J. MURRAY.

P.S. It should be stated, that when the red-hot platinum wire is introduced into a wine-glass containing sulphuret of carbon, it always kindles the fluid; but it will be seen to continue red hot between the surface of the fluid and the base of the flame.

When oil is used in the cell of the blowpipe it has this disadvantage: the ebullition is distinguished with much difficulty by the ear; but when *water* is employed, the bubbling is *very audible*,—and the latter is therefore a less equivocal index of safety. J. M.

XXXVI. *Observations on the Combination of Metals with Sulphur. By M.* A. J. FRERE DE MONTIZON*.

A PARTICULAR examination which I have made of cinnabar, leads me to suppose that this sulphuret results from the combination of a volume of mercury and another of sulphur: but in order to confirm my opinion in the most positive manner, I have endeavoured to ascertain whether this mode of composition be also that of other sulphurets.

On this subject I have had recourse to the analyses generally adopted,

* From the *Annales de Chimie et de Physique*, for September 1816.

adopted, and fixed my attention to these alone, as calculated to inspire more confidence in this sort of investigation.

The results which I have obtained confirmed my remark, by some thousandth parts of sulphur in 100 parts of mass. Cinnabar offers the greatest difference:—according to Berzelius I have found about a hundredth of sulphur in 100 parts in weight; and according to Seguin it is less by half a hundredth.

As my experiments on this occasion may possess some interest on account of science, I have ventured to offer the following table of sulphurets.

Sulphurets.	Authors of the Analyses.	Density of the Metals.	Quantity of Metal. In weight Sulphur =20.	Quantity of Metal. In volume Sulphur =10,05.	Reduction of volumes. Sulphur.	Reduction of volumes. Metals.
Of Mercury	Berzelius	13·599	125·5	9·227	1·	1 or 0·918
— Mercury	Seguin	13·599	131·26	9·652	1·	1 - 0·960
— Tin	Berzelius	7·291	73·5	10·081	1·	1 - 1·003
— Nickel	Proust	8·279	42·55	5·140	2·	1 - 1·002
— Arsenic	Laugier	8·308	27·62	3·324	3·	1 - 0·992
— Molybdenum	Bucholz	7·400	30·	4·054	5·	2 - 2·019
— Zinc	Gay-Lussac	6·861	41·	6·121	5·	3 - 3·045
— Cobalt	Proust	8·5384	50·	5·855	5·	3 - 2·913
— Antimony	Berzelius	6·7021	53·69	8·011	5·	4 - 3·986
— Silver	Marcet	10·4743	135·	12·889	4·	5 - 5·130
— Lead	Berzelius	11·224	129·5	11·538	7·	8 - 8·036
— Bismuth	Berzelius	9.7654	86·35	8·843	8·	7 - 7·039
— Copper	Chenevix	8·895	40·	4·497	9·	4 - 4·027
— Iron	Thenard	7·788	34·5	4·430	9·	4 - 3·967

XXXVII. *Notices respecting New Books.*

A Dissertation on Weights and Measures, and the simplest Means of revising the British System,—said to be from the pen of Dr. Gregory of Woolwich, and printed in the 17th number of the British Review,—will be published in a separate pamphlet immediately.

Early in the Spring will be published by Mr. Newman, Soho-Square, Chromatics; or An Essay on the Analogy and Harmony of Colours; containing elementary Instruction for producing Colours by Composition in all the Variety of Hues and Shades; with a new Theory of their Relations, Arrangement, and Harmony.

In this Essay the coincidences of Music will be pointed out; and the whole illustrated by coloured diagrams, &c.

Mr. Murray's Elements of Chemical Science, second edition, with

with additions, is in the press, and will be forthwith published by Messrs. Underwood of Fleet-street.

This edition will contain a succinct and lucid view of those discoveries which have of late distinguished the rapid and brilliant march of chemical science.

The article of *safety-lamps* for mines, and account of experiments made by the new blow-pipe with a condensed mixture of oxygen and hydrogen, will possess considerable interest.

The publication of Dr. Spurzheim, containing a Reply to the antagonists of his doctrine, so long expected, is at length issued from the press in Edinburgh. It contains a full answer to the various objections raised against his new views of the functions of the brain by the several Reviews, and by Dr. Gordon in his late pamphlet on this subject.

The scientific reader will be less interested in the severe animadversions of Dr. Spurzheim on his adversaries, than in the excellent summary of his peculiar doctrines, which he has compressed in so small a compass, in the course of a series of explanations and answers to objections which the late controversy had given birth to.

M. Fant, Professor of History in the University of Upsal, has published the prospectus of a work to be entitled *Scriptores Rerum Suecicarum Medii Ævi*. There has been long wanting in Sweden a collection of the chronicles, diplomas, and other historical monuments of the middle age. The late King Gustavus III. gave orders to M. Nordin, bishop of Hernæsand, to collect the various monuments scattered in different archives and libraries, some in MS. and others inaccurately printed. M. Nordin proceeded to Stockholm, and occupied himself for ten years with this work; but having returned to his diocese after the death of Gustavus III. he could not continue his enterprise, and the materials which he had collected remained in his private library. At his death his heirs wished to sell at a high price this important collection: the Prince Royal bought it, and presented it to the library of the University of Upsal. The reigning King of Sweden has ordered it to be published at the expense of the State. M. Fant has been directed to revise it, to complete it, and to give an edition similar to that of Langebeck's collection entitled *Scriptores Rerum Danicarum*. This collection of the historians of Sweden will therefore soon appear: the first volume is in the press; it will consist of 150 sheets;—the entire work will fill three volumes in folio, and the price of each volume will be only about fifteen shillings English.

A small volume upon the Art of making, managing, flavouring, colouring, preserving, and recovering all Kinds of Wines, Spirits, and Compounds, with Directions for Brewing, &c. by M. R. Westney, will be published in a few days.

XXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 6. THE President in the chair. A paper by Ed. Davy, Esq. Professor of Chemistry at the Cork Institution, was read, relating his discovery of a fulminating platinum. Thin plates of platinum were dissolved in nitro-muriatic acid, the solution evaporated to dryness, and afterwards boiled with potash and ammonia, in nearly the same manner as the aurum fulminans is prepared, when the author found a gray powder which explodes violently on exposure to heat. On its explosive powers Mr. Davy made a great variety of experiments, and also to ascertain with precision its real nature and constitution. On heating the gray powder with lime in a retort, ammonia and water were produced; one grain yielded 0·15 cubic inch of nitrogen gas; but it appeared that, in consequence of the formation of water, about 0·03 of this gas had been absorbed.

Feb. 13. Continuation of Mr. Davy's paper. The author analysed every product with great care, and investigated all the new or peculiar appearances with unusual fidelity and minuteness, in order to discover accurate data for a correct rationale of this new fulminating powder, which he considers as a triple salt, composed of oxide of platinum and ammonia. It explodes gently at the temperature of 300 Fah. and violently at 400; whereas the aurum fulminans explodes violently at temperatures between 120 and 300. From a great number of experiments Mr. D. states that 100 grains of this fulminating platinum consists of oxide of platinum 82·5, of which 72 are metal and 10·5 oxygen; of ammonia 9, and water 8·5 = 100.

Feb. 20. The Astronomer Royal communicated some of his Observations on the Parallax of the fixed Stars. Mr. Pond has not yet completed the series of his observations; but from what he has already observed he is inclined to dissent from Dr. Brinckley's opinion, and to question the existence of any parallax in certain of the fixed stars, in consequence of finding the changes so very small.

ROYAL

ROYAL SOCIETY OF EDINBURGH.

Since our last Report different interesting communications have been made to the Society by the indefatigable Dr. Brewster, "On the phænomena of optical contact; the inflexion of light; the colours of thin plates; the production of nebulous images by doubly refracting crystals; the distribution of the polarising force in tubes, cylinders, and plates of glass; and the effects of mechanical pressure in communicating double refraction to regularly crystallized bodies; also a very extensive series of experiments on the action of regularly crystallized bodies upon light."

A paper by the Rev. Dr. Fleming, "On the junction of the fresh-water of rivers with the salt-water of the sea," details a number of experiments made in the Firth of Tay, with an apparatus by which water could be brought from any given depth. It appears from these experiments, that when the flow of a river is obstructed by the tide, the salt-water slides under the fresh like a wedge, and sends the fresh to the surface.

Dr. Henry Dewar exhibited some words formed in relief for the use of the blind and deaf lad Mitchell, in whose instruction the Society has taken a laudable interest.

A letter from Professor Playfair presented "An account of some appearances on the sides of the mountains in Switzerland, considered by the writer as analogous to the parallel roads of Glenroy in Scotland." They consist of lines extending nearly horizontally for miles, and on inquiry were found to have been formed for the purpose of irrigation; whence it is inferred that those of Glenroy may have had a similar origin.

Mr. Bald, civil engineer, communicated "Experiments made with Sir H. Davy's safety-lamp in mines in Ayrshire." The results agree perfectly with those made in other mines, and already laid before our readers.

A communication by Mr. Bonar presented "Observations on the filiation of the various languages of the eastern part of India, and on their affinity with the Sanscrit and the Chinese."

SOCIETY OF LEGHORN.

An Academy of Sciences, Letters and Arts has just been established at Leghorn. It held its first sitting in the Hall of the Palace of Pullie, and was attended by all the public functionaries, many strangers of distinction, and several of the professors of Pisa. Agreeably to the desire of His Highness the Grand Duke, who has honoured the Society with his patronage, it is every month to publish the

best memoir which it may receive from members or from foreign associates upon *political œconomy;* and under the title of the *History of Political Societies,* it will from time to time publish notices upon the governments of the different European States.

FRENCH ROYAL ACADEMY OF SCIENCES.

Sitting of the 9th Sept. 1816.

MM. de Lacépède, Geoffroy-Saint-Hilaire, and Dumeril made a Report to the Academy upon the *Monographie des Trigonocephales des Antilles* of M. Moreau de Jonnès.

The serpent which is the subject of the memoir of M. Jonnès is of a large size, and its bite is very dangerous; it has been seen more than eight feet long. The author affirms that it is confined to the islands of Martinique, St. Lucia and Baconia, and that it has never been observed on the continent of America.

It is well known that the species of vertebral animals which see better by night than by day, or which are very sensible to light, present in general a vertical pupil. M. Jonnès has observed the same disposition in the iris of the *trigonocephalus* which he describes. This animal, whose agility is very remarkable, has a peculiar manner of darting itself. It curls up its body into four equal circles, one above another, and these making their circumvolutions all at once, it projects itself thus in mass to the distance of five or six feet. Another fact which M. de Jonnès mentions, is, that the *trigonocephalus* can in the manner of the *najos* rear itself upon its tail to the height of a man. He assures us further, that by means of certain large scales with which the belly of this reptile is covered, it can like some adders climb up the trunks of trees and along the branches in search of birds-nests, the young of which it devours. The most efficacious means for preventing the fatal consequences of the bite of this serpent, are the same as have been used in Europe with more success in opposing the development of hydrophobia.

Sitting of the 23d Sept.

M. Aymez announced that he had discovered an indelible ink. The examination of the discovery was remitted to Messrs. Deyeux and Thenard.

M. Hallé read a Report from the Commission appointed to examine the Memoir of M. Majendie "On the nutritive Properties of Substances which do not contain Azote."

After a detailed analysis of the interesting experiments of the author, the reporter added, "It still remains, however, setting out

out from the point at which M. Majendie has arrived, to examine in what proportions the mixture of alimentary substances which contain azote, with such as are deprived of it, may be sufficient or insufficient to complete nutrition, to repair entirely the waste which takes place, and to maintain the animal in a degree of health and of strength corresponding to his organization. M. Majendie will without doubt accomplish all this; and in doing so he will effect the solution of a problem of the greatest importance to the animal œconomy, to medicine, and in particular to the theory of regimen."

XXXIX. *Intelligence and Miscellaneous Articles.*

FIGURE OF THE EARTH.—PENDULUM.

The question respecting the figure of the earth, and the anomalies in the English and other trigonometrical surveys, which has given rise to so much interesting discussion during the last three or four years, is likely to receive considerable illustration in the course of the present. The gentlemen engaged in the trigonometrical survey, having purchased expressly for this purpose, about three years ago, a fine astronomical clock, (made by Pennington,) intend taking it to the Orkneys with other apparatus, as soon as the season is sufficiently advanced, to ascertain the vibrations of pendulums at that high northern latitude. MM. Biot and Arago are deputed from the French Academy to meet them there with the pendulum apparatus which has already been employed on the arc of the meridian between Dunkirk and Formentera. And it is hoped that another deputation from the Royal Society will join the party. The same respective sets of apparatus will be carried to several points between the Orkneys and Blackdown (near Weymouth); and thus in connexion with the results already obtained on the continent, being spread over an arc of nearly 21 degrees, must furnish some extremely interesting data for the future investigations of mathematicians and philosophers.

INDENTATIONS IN THE SUN'S EDGE.

Several years ago Dr. Herschel, after tracing a solar spot across part of the sun's disk, until it passed off its edge, observed it to occasion there an indentation; and thereby he made the important discovery, that the spots on the sun are depressions in the surface, or holes through the luminous atmosphere, by which the

sun is surrounded.—Now although solar spots are not of uncommon occurrence, their magnitudes and motions, compared with the sun's disk, are so small, and the chances, during the sun's appearance, of observers being at their telescopes at the proper times for seeing a spot, or hollow of sufficient depth, on the apparent edge of the sun, are in consequence so small, that Dr. Herschel has not more than once or twice since, been able to repeat his observations on *the indented edge of the sun*, and numerous telescopic observers have never yet been gratified by this curious sight.

It happened on Friday, January 31st last, at one o'clock in the afternoon, that two hollows, producing indentations, were at the same time coming on the sun's disk, in the lower right-hand quarter, as seen through a *reversing* telescope with a magnifying power of 100 times. Mr. Henry Hubert, coal-merchant, in Little Abingdon-street, Westminster, was then trying an excellent four feet Dollond's achromatic, which he had recently purchased, searching for small spots on the sun's disk; and he saw, near together, two conspicuous and large indents in the otherwise remarkably true and circular disk of the sun, as mentioned above; that which appeared lowest was by far the broadest and deepest indent, extending to a great depth upon the face of the sun's disk (beyond the apparent loss of substance therein); the bottom or inner edge of them appeared irregular, and not circular. There were a number of small spots, about ten or twelve in a group, at a small distance from the above indentations. Mr. Hubert was in hopes of being able to repeat his observations some days afterwards, but clouds intervened at the time of the indentations passing off the opposite edge of the sun's disk.

STEAM ENGINES IN CORNWALL.

The average work of 27 engines, reported by Messrs. Lean, for January, was 23,373,320 pounds of water lifted one foot with each bushel of coals consumed.

During the same month Woolf's engine at Wheal Abraham, loaded 14·9 pounds per square inch in engine cylinder, lifted 43,368,479 pounds with each bushel: his other engine at the same mine, loaded 3·1 per square inch, lifted 25,505,239 pounds per bushel; his engine at Wheal-Vor, loaded 15·2 per inch, lifted 39,295,441 pounds: and his engine at Wheal-Unity, loaded 12·5 per inch of cylinder, lifted 26,154,555 pounds.

The Wheal Chance engine during January, loaded 13·2, lifted 43,840,102 pounds per bushel.

AGRICUL-

AGRICULTURE.

The following letter, addressed by the Earl of Albemarle to the Norfolk farmers, is of much importance to agriculturists in general:

"Gentlemen,—Having made some experiments relating to the probability of a good crop next harvest, to be grown from black barley used for seed, I think it cannot be uninteresting to you to be acquainted with the results. On Saturday the 18th of January I planted two hundred kernels of three different samples of barley, in separate pots filled with mould taken from a turnip-field (the turnips of which had been fed off by sheep), which came in course for barley. The kernels were not picked, but taken as they came to hand. The pots were placed in a hot-bed—No. 1 was a sample of bright barley, rather high coloured, having been got up dry, but partially wetted upon the stack before it was thatched. This barley was sold at Harling market on the 21st of January at 29*s.* 6*d.* per coomb.—No. 2 was a sample of black barley, of the quality of the black barley generally, this year sold at Diss market on the 24th of January at 12*s.* per coomb.—No. 3 was a sample of black barley, which had been still more exposed to the rain than No. 2. I am not aware that any part was grown barley. On the eighth day from the sowing (Saturday the 25th of January) I examined the pots, and found that of two hundred kernels sown in pot No. 1, one hundred and three kernels appeared in a state of vegetation: of two hundred kernels sown in pot 2, one kernel appeared in a state of vegetation; and of two hundred kernels sown in pot No. 3, two kernels appeared in a state of vegetation. On the 15th day from the sowing (on Saturday the 1st of February) I again examined the pots, and found that of two hundred kernels sown in pot No. 1, one hundred and ninety-seven kernels appeared in a state of vegetation; of two hundred kernels sown in pot No. 2, thirty kernels appeared in a state of vegetation; and of two hundred kernels sown in pot No. 3, eighteen kernels appeared in a state of vegetation. The few plants which showed themselves in Nos. 2 and 3 came up very irregularly, and looked unhealthy, so much so, that it was the opinion of those who examined them with me, that had not particular care been taken, and had the barley been exposed to too much rain or drought, scarcely so many would have vegetated. On the twenty-second day from the sowing (on Saturday the 8th of February) I examined the pots for the third time, and found that of two hundred kernels sown in pot No. 1, one hundred and ninety-seven kernels appeared in a state of vegetation; of two hundred kernels sown in

 pot

pot No. 2, thirty-six kernels appeared in a state of vegetation; and of two hundred kernels sown in pot No. 3, nineteen kernels appeared in a state of vegetation. I hope this statement may serve as a caution to such of you (and I understand the number is very considerable) who are disposed to trust to the black barley for seed; or at least that your attention may be drawn to a subject of so much importance to our county. It is in the power of every one of you to prove the correctness of this statement by making the same experiments.

"I am, gentlemen, your sincere friend,

"Lindenham, Feb. 10, 1817. "ALBEMARLE."

SAFETY-LAMPS.

We have received a communication from the Rev. Mr. Hodgson on Mr. Stevenson's pretended claims to this invention, to which, though many of the facts have been stated in our pages, we would willingly have given a place had it reached us in time; but we did not receive it till the 24th February (and the publication must be out by the 28th). Independently of this, the publication to which he has alluded does not appear to us to deserve the serious answer which he has given to it, by again bringing forward all the dates. All the facts that have transpired, which have been published by Mr. Stevenson's friends, convince us that his attempts at safety tubes and apertures were borrowed from what he had heard of Sir Humphry Davy's researches. One paragraph, however, in Mr. Hodgson's communication deserves particular attention, and therefore we insert it here:

"I think it necessary (says Mr. H.) to notice that I have had no authority from Sir H. Davy to publish extracts from his letters; but that I was obliged to do so in justice to myself: for malicious persons might have stated that hints from these letters had been clandestinely conveyed to Mr. Stevenson. That this was not the case, is to be found in the fact that safety apertures and tubes were announced to persons concerned in the N. Castle coal mines, before Mr. Stevenson had made an experiment on more tubes than one, or on apertures of any kind."

COMPARATIVE TEMPERATURE OF RAIN IN 1815 AND 1816.

In the last number of the *Bibliothèque Universelle* there is the following curious comparison between the temperature of the rain at Paris in the years 1815 and 1816. In 1815 the mean temperature of the first ten months was $= + 12 \cdot 0^\circ$ (centigrades). In the year 1816 it was $= + 10 \cdot 5^\circ$. In 1815 the quantity of rain collected during the first ten months was $= 36$ cent. 77. In 1816

1816 it was = 43 cent. 47. The difference therefore in old measures corresponds to two inches five lines and seven tenths. It is added that in 1815 there were from January to October inclusive, 127 days of rain; and in 1816 during the same space of time *eight* days more. In 1815 it rained twelve times in July; in 1816 the same month presented only *five* days without rain.

EXPEDITION TO AFRICA.

The Congo arrived at Spithead from Bahia on the 23d of February, having brought home the whole of the crew that embarked on the expedition to Africa, except one, who died at sea of the flux. Mr. Lockhart, from the King's Garden at Kew, and the serjeant of marines, were left at Bahia, not being considered as sufficiently recovered from the sea voyage.

CLARIFYING OF SUGAR.

The *Annales de Chimie* xcv. 232, contains a notice, that a Mons. Dorion having discovered that the bark of the pyramidal ash, in powder, thrown into the boiling juice of the sugar-cane, effects its clarification, the planters of Guadaloupe had given him 100,000 francs, and those of Martinique a like sum, for communicating his discovery; and that the English sugar planters had bought the secret for 4000 francs.

Dr. Thomson (Annals of Philosophy, No. 49), alluding to this notice, states, that he had learnt from a planter lately returned from the West Indies, that this process had been known there for years; that the planter had himself employed it, but never heard M. Dorion's name mentioned, " and is quite sure that the alleged purchase of the method by the English planters is not true."

In the statement of Dr. Thomson's friend, as also in that of the *Annales de Chimie*, there is a mixture of truth and error, which by the favour of an intelligent correspondent we are enabled to clear up. It is true that the Doctor's friend " from the West Indies" (rather a wide range, however, when facts depend upon locality) " never heard M. Dorion's name mentioned there; for the name of the person who made the discovery is not Dorion, but Du Boc—and however sure he may be " that the alleged purchase of the method by the English planters is not true," we are well assured that the House of Assembly of Jamaica voted M. Du Boc one thousand pounds sterling for his secret. As this discovery is an important one, and the process has not been before published in this country, we are happy in being able to lay the following communication before our readers:

To

To Mr. Tilloch.

Sir,—In reference to the article in Dr. Thomson's Annals of Philosophy, last number, on the clarifying of the syrup of the sugar-cane, permit me to submit to you the pertinent remarks of an intelligent friend of mine lately returned from Jamaica. He was with Mons. Du Boc at the moment when he received, on the 13th of December 1815, the Kingston Gazette announcing the award of the House of Assembly noticed in the subjoined statement.

I always am, sir,
Yours most obediently,

Surry Institution, Jan. 7, 1817. J. Murray.

"The use of the bark of the *bastard cedar* (a tree very common in all or most of the West Indian islands) was first introduced in the purification of sugar by M. Du Boc, by birth a Frenchman, and who had been a planter in the island of Martinique, where it appears he had with great success practised the use of it. M. Du Boc went to Jamaica in the early part of 1815, where he was patronized by several planters of eminence, to whom he individually explained his discovery. The House of Assembly of Jamaica were so thoroughly convinced of the benefits accruing from this application of the bastard cedar bark, that in the month of October 1815 they voted him a thousand pounds sterling.

"The process is extremely simple, and consists merely in the immersion of a few strips of the bark (peeled off a branch of the tree) in a bucket of water, and by squeezing the bark with the hand in a short time the water becomes gelatinous, and it is then thrown into the copper in which the sugar is boiling. Soon after this is thrown in, the surface of the boiling sugar is covered with a thick black scum, which consists of the solution of the bark intermixed with those impurities of the sugar which the lime does not precipitate—that scum is removed with a scummer in a few minutes. The sugar is afterwards drawn off from the copper into the cooler, and is then considered as divested of every impurity.

"There grows also in Jamaica a tree, the leaf of which used in the same manner produces the like effects. It is known in Jamaica by the name of *the Whangler* or *Wangla*, but I know not its specific or generic name."

SWALLOWS.

On the 17th of February, about nine in the morning, Mr. Thomas Forster observed the chimney swallow (*Hirundo rustica*) flying about at Tunbridge Wells; which accords with the notion entertained by some ornithologists, that as these birds sometimes

times make their appearance unusually early in early seasons, there are probably some of the species latent in this country during the period of their brumal disappearance.

AURORA BOREALIS.

This phænomenon appeared again on Saturday night the 8th inst. on which observations have, we understand, been made in London, at Derby, Leeds and other places, and also at Paris.

Leeds, Feb. 15.

"On Saturday the 8th inst. at seven o'clock we witnessed here a fine display of the *aurora borealis* or northern lights. This singular phænomenon continued without intermission for two hours, during the whole of which time the sky was illuminated from the horizon to the zenith, extending east and west for a considerable distance. Broad streaks of light of various sizes rose from the horizon in a pyramidical, undulating form, and shot with great velocity up to the zenith; they changed their forms very frequently and rapidly, and broke out in places where none were seen before, shooting along the heavens, and then disappearing in an instant. The sky in various places was tinged for a considerable space with a deep purple, and the stars shone very brightly during the whole time through the clouds which formed the *aurora borealis.* A short time after this singular phænomenon had ceased, the rain began to descend, and continued to do so most of the night, though not violently."

Among the many indications of an early spring, after nearly a year of unusually wet and changeable weather, may be enumerated the early blowing of spring flowers. On the 28th of January the spring snow-flake and crocus were in full flower in the neighbourhood of Hackney.

SPEAKING TRUMPET.

If the speaking trumpet used at sea had been made in any other form than that of a parabolic conoid, it would have failed greatly in its effect. It is surprising that this principle should not have struck any one as the proper model for forming a trumpet for the collection of sound in cases of deafness. Mr. Curtis of Soho-square, aurist to his royal highness the Prince Regent, has with much advantage applied this principle to a late contrivance, and employed the instrument with great success in a number of cases of deafness: the same gentleman has also improved the French invention of artificial ears, by giving them great power in the collection and transmission of sound.

LIST OF PATENTS FOR NEW INVENTIONS.

To Joseph de Cavaillon, of Sambrook-court, Basinghall-street, London, for certain improvements in the preparing, clarifying, and refining of sugar, and other vegetable, animal, and mineral substances, and in the machinery and utensils used therein.—6 months, 23d January 1817.

To Robert Dickinson, of Great Queen-street, Lincoln's-inn-fields, in the county of Middlesex, for his new improved method or methods of preparing or paving streets and roads for horses and carriages, so as to render the parts or pavements when so done, more durable and ultimately less expensive than those in common use, and presenting other important advantages.—6 months, 23d January.

To Daniel Wilson, of Dublin, for certain improvements in the process of boiling and refining sugar.—6 months, 23d Jan.

To George Montague Higginson, of Bovery Tracy, Chudleigh, for certain improvements in locks.—2 months, 1st Feb.

To William Wall, of Wandsworth in the county of Surry, for his new or improved horizontal escapement for watches.—2 months, 1st February.

To Isaac Robert Mott, of Brighton, for his method of producing from vibrating substances a tone or musical sound, the peculiar powers in the management whereof are entirely new, and which musical instrument he denominates The Sostinente Piano-forte.—2 months, 1st Feb.

To William Bundy, of Pratt-place, Camden-town, in the parish of St. Pancras, for machinery for breaking and preparing flax and hemp.—6 months, 1st Feb.

To James Atkinson West, of Crane-court, Fleet-street, London, for certain improvements in or on lustres, chandeliers, and lamps of various descriptions, and in the manner of conveying gas to the same.—6 months, 6th Feb.

To William Clark, of the city of Bath, for his contrivance called a safeguard to locks, applicable to locks in general, by which they may be so secured as to defy the attempts of plunderers using picklocks or false keys.—2 months, 8th Feb.

To Robert Hardy, of Worcester, for certain improvements in the manufacturing of cast iron bushes or pipe boxes, for chaise, coach, waggon, and all other sorts of carriage wheels.—6 months, 20th Feb.

To Richard Litherland, of Liverpool, for certain improvements in or on the escapement of watches.—6 months, 20th Feb.

To Richard Holden, of Stafford-street, in the parish of St. Mary-le-bone, for his machines for producing rotatory and pendulous motion in a new manner.—6 months, 20th Feb.

Astro-

Astronomical Phænomena, March 1817.

D. H. M.
1 21.20 ☽ γ ♌
3.15·56 ☽ ν ♍
4.17 45 I } of γ ♍ * 12′ S ☽
4.18.19 E } cent.
5. 0· 0 ☽ in perigee.
6. 8.46 ☽ κ ♍
7.23.20 ☽ κ ♎
8. 3.50 ☽ λ ♎
8. 9 29 ☽ β ♏
9.15.56 θ ophiuchus.
9. 0 50 ☽ ♃
10.17.42 ☽ λ ♐
11. 1.29 ☽ φ ♐
12. 0. 0 ♀ 27 ♈ * 7′ S.

D. H. M.
16. 0. 0 ♄ ☿ , ☿ 26′ S. of ♄.
20. 0. 0 ♀ 47 ♈ * 14′ N.
20. 0. 0 ☽ in apogee.
20.10.54 ☉ enters ♈.
21. 0. 0 ♅ stationary.
21.14. 0 ☾ ♀
21.14.27 ☾ 125 ♉
24 18.28 ☾ 132 ♉
25.18 23 ☾ ε ♉
26.19 50 ☾ κ ♊
28. 0.0 ♃ stationary
29. 6.35 I } of η ♌, * 10 N of
29. 7.37 E } ☽'s centre.

Meteorological Observations kept at Walthamstow, Essex, from January 15, *to February* 15, 1817.

[Usually between the Hours of Seven and Nine A.M.]

Date.	Therm.	Barom.	Wind.
January			
15	32	29·39	S.SE.—Snowing 8 A.M.; ground whitened; 12, rain; bright starlight.
16	21	29·33	E.—8 A.M. hazy; snow; rain; dark and damp.
17	39	28·92	S.—Hazy; gleams of sun; shower 4 P.M.; bright star light. Full moon.
18	42	29·00	S.—Cloudy; sun and clouds; fine day; star light.
19	42	29·01	SE.—Clear and clouds; and wind showers; cloudy.
20	46	28·88	S.SE.—Clear; clouds; wind and sun; fine day; slight rain 6 P.M.; dark night.
21	32	29·42	SE.—7 A.M. clear; 8 hazy; 9 fine day; starlight.
22	44	29·88	S.—Slight rain; windy and hazy; slight rain again; dark and windy.
23	49	29·92	S.—Hazy day; dark night.
24	47	30·21	WS.—Clear, but some *cirrostratus* N.W.; hazy; dark and damp.
25	49	30·33	S.—Foggy; hazy; hazy night. First quarter.
26	46	30·33	S.—Clouds; wind and hazy; fine gray day; *nimbus* all day; cloudy and hazy.
27	46	30·30	W.N.—Cloudy and hazy; dark day; cloudy and *nimbus* all day; very light, but no stars visible.

Date.	Therm.	Barom.	Wind.
28	43	30·44	N by E.— Damp; hazy; *nimbus* all day; cloudy.
29	42	30·32	NE.—Clear and clouds; *cirrostratus;* fine day without sun till 3 P.M.; light, and moon through clouds.
30	43	30·32	NE.—Hazy and slight rain; gray day; moon and stars and *cirrostratus.*
31	44	30·43	N.—Foggy; fine sunny day; moon and stars and *cirrocumuli.*
Febuary			
1	37	30·54	N.—Hazy with frost; fine day; clear.
2	41	30·44	N.—*Nimbus;* fine gray day; light, but neither moon nor stars visible. Full moon.
3	39	30·43	W.—Foggy; cloudy day; cloudy.
4	40	30·00	S.—Foggy; sun through clouds; *cumuli*; and moon light.
5	40	29·61	NW.—Clear and wind; slight *cirrus* NW.; fine day; rain.
6	50	29·95	W.—Clear and wind; fine day; star light..
7	44	30·10	W.—Clear and clouds; and wind; fine beautiful red and orange sun set; windy and dark.
8	45	30·30	W.—Cloudy and windy; fine day; at 7 P.M. bright light in the north-eastern horizon, which appeared like the full moon just going to rise; still brighter at 10 P.M.; cloudy and windy. Moon last quarter.
9	44	30·20	W.—Cloudy and wind; very fine day; star light; the same cloudy clear *aurora borealis* as last night, but not so bright.
10	49	30·20	W.—Gray fine day; sun and clouds; dark night.
11	39	29·30	NE.—7 A.M. rain; 8 snow till 11; after that fine day; sunshine; star-light.
12	41	29·43	S.—Sun and wind; showers and sun; stars; and the *light* NW. *high.*
13	39	29·82	SW.—Foggy; damp cloudy day; some rain NW.; dark; the lights again NW. at 10 P.M.
14	40	29·43	NW.—Wind and cloudy; very fine day; sun and wind; bright star light.
15	40	29·53	S.NW.—Rain and foggy; very fine day; star light.

A correspondent writes, that the neighbourhood of Bushey and Harrow-on-the-Hill has been involved during the last week in a constant *nimbus,* forming small rain close to the ground. —28*th Jan.* 1817.

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1817.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
1817.	DAYS.			
Jan. 16	29	29·5	29·22	Snow—rain P.M.
17	new	38·	28·97	Fine
18	1	42·5	29·16	Cloudy
19	2	44·	29·05	Ditto—heavy rain at night
20	3	49·	28·87	Fair—blows hard from S—slight frost at night.
21	4	38·5	29·65	Very fine—heavy rain at night
22	5	52·	29.80	Ditto—blows very hard
23	6	53·	29·77	Fair ditto
24	7	54·5	30·35	Cloudy
25	8			
26	9	49·	30·25	Cloudy
27	10	45·	30·48	Very fine—rime frost at night
28	11	41·5	30·47	Fair—rain P.M.
29	12	41·5	30·38	Very fine
30	13	51·5	30·38	Ditto
31	14	49·5	30·52	Ditto—rime frost at night
Feb. 1	15	46·5	30·60	Ditto ditto
2	full	43·	30·57	Ditto ditto
3	17	37·5	30·35	Foggy ditto
4	18	44·5	29·89	Fine—heavy gale from NW at night
5	19	38·	29·84	Fair—a little rain at night
6	20	59·5	30·02	Fine
7	21	52·5	30·20	Fair—blows hard from W
8	22	51·	30·24	Ditto ditto—In the evening a very brilliant and beautiful exhibition of the *aurora borealis*
9	23	45·	30·40	Fair—blows hard from W
10	24	51·5	30·10	Ditto ditto—rain in the evening; snow and sleet in the night
11	25	42·	30·15	Ditto—snow at night [and rain
12	26	46·5	29·52	Rain—frost in the evening—fog
13	27	42·	29·80	In the morning rain

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For February 1817.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Jan. 27	42	44	44	30·30	6	Cloudy
28	44	46	44	·20	0	Cloudy
29	44	47	44	·18	7	Cloudy
30	44	54	46	·12	5	Cloudy
31	45	52	40	·37	12	Fair
Feb. 1	40	54	42	·37	16	Fair
2	42	44	40	·37	10	Cloudy
3	40	42	40	·16	11	Cloudy
4	39	45	42	29·70	22	Fair
5	39	45	42	·80	36	Fair
6	48	54	44	·90	23	Fair
7	44	52	47	30·07	37	Fair
8	47	51	45	·12	38	Fair
9	45	50	46	·16	30	Cloudy
10	47	50	45	29·94	24	Cloudy
11	45	42	34	·90	0	Snow showers
12	40	43	38	·50	24	Showery
13	38	45	39	·56	23	Showery
14	39	48	42	·52	38	Fair
15	42	52	40	·42	41	Fair
16	42	48	40	·70	42	Fair
17	42	55	50	·82	21	Cloudy
18	50	54	50	·98	11	Cloudy
19	40	48	46	30·10	42	Fair
20	45	49	50	29·50	7	Cloudy
21	35	46	35	·42	38	Fair
22	42	47	42	·67	37	Fair
23	41	47	49	·85	9	Cloudy
24	42	49	45	·82	35	Cloudy
25	43	52	46	·89	15	Cloudy
26	43	47	46	·70	46	Fair

N.B. The Barometer's height is taken at one o'clock.

XL. *On the Agency of the Carbonate of Magnesia in improving Bread made from the new Flour.* *By* Edmund Davy, *Esq. Professor of Chemistry, and Secretary to the Cork Institution.*

To Mr. Tilloch.

Dear Sir,—In a recent communication*, I stated that new seconds flour, of indifferent or bad quality, is materially improved for the purpose of making bread, when the common carbonate of magnesia is well mixed with it in the proportion of from 20 to 40 grains to a pound of flour.

Since I announced this fact, I have made a number of comparative experiments on the worst seconds flour I could procure, with and without the addition of the magnesia; and the results have uniformly been satisfactory. The efficacy of this substance has also been repeatedly proved by trials made in Cork, and in different parts both of Ireland and England.

In a few cases, however, it has been said, the magnesia failed to produce the desired effect on the new flour; at which I am not surprised; for to my knowledge the *calcined* magnesia has in some instances been used instead of the *common carbonate*, and there is too much reason to apprehend this last substance has in other cases been adulterated by admixture with foreign bodies. In an early stage of my experiments, I found the calcined magnesia (when used in the quantity of from 20 to 30 grains to a pound of flour) to injure the colour of the bread and to render it heavy: and in the proportion of 40 grains to a pound it even changed the colour of the dough, and made it assume a yellow hue, not unlike that tint imparted by saffron. In the proportion of 12 grains to a pound of flour, however, the *calcined* magnesia improved the bread, but not nearly to the same extent as the *carbonate*.

There certainly may exist a difference of opinion as to the quantity of improvement effected in the bread by the magnesia. Slight circumstances, by no means easy to appreciate, may in different cases materially alter the nature of results. But no one who has fairly tried the magnesia, in the way I have recommended, can hesitate to admit the fact. I venture to speak confidently from experience. I do it under the full conviction that, whilst too much caution cannot be exercised in drawing conclusions from one or two hasty trials, the most legitimate inferences may be deduced from experiments carefully made and frequently repeated.

In the communication to which I have alluded, I merely hinted at the probable agency of the magnesia in correcting the bad qualities of the new flour. I now beg leave to notice the circumstances

* See Phil. Mag. for December 1816.

cumstances that led me to use this substance, and to bring forward some facts that tend to elucidate the mode of its operation.

On examining several samples of new wheat, it appeared to me the injury they had sustained arose principally from the grain having germinated. In this process it is understood a part of the farinaceous matter of the grain is converted into sugar. The changes thus produced in the grain seem to be analogous to those effected in starch by diluted sulphuric acid and heat: only in the one case they result from chemical agencies alone; in the other, the chemical agencies are assisted by the living powers of the infant plant. The production of saccharine matter in the grain is conceived to be accompanied by an incipient fermentation: this, though checked in the act of drying the corn, might, in the subsequent operations to which the flour was exposed when made into dough and baked, induce the acetous fermentation. Hence, I thought it not improbable that acid matter might be developed in bread made from the new flour with the usual additions.

In order to try if the new flour was at all acid, I put samples of the worst quality in distilled water; but after several hours the fluid did not affect litmus paper, or the still more delicate test of an infusion of red cabbage. I likewise exposed fermented dough made from the same flour (with the additions of yeast, salt, and warm water,) to an infusion of cabbage; but no change of colour could be perceived in it. But after some of the dough had been baked, the bread was clammy, had a singular smell, a sourish taste, and left on the palate a sense of bitterness. When some of this bread was put in distilled water and suffered to remain for about a day, the fluid produced a slight tint of red in an infusion of cabbage; and after three days it had a slight acid taste, and perceptibly reddened litmus paper. On making a comparative trial with bread made from good old flour, under similar circumstances, I could not by the most delicate tests detect the presence of any acid. Hence it seemed acid matter was formed in the baking of the new flour, and the bread had a tendency to acidity. I found likewise, in cases when a few drops of vinegar, or any of the mineral acids diluted, were put into dough made from good old flour, the bread was disposed to be clammy, and had a taste similar to that of bread made from bad new flour. These circumstances led me to imagine that the bad qualities of bread made from the new flour were connected with the appearance of acid matter during the baking: and the application of alkaline substances, as correctives, immediately became obvious. After repeated trials, I found that the fixed and volatile alkalies, their subcarbonates and carbonates, improved the new flour to a certain extent; and

I was

I was in consequence led to try the effects of the common carbonate of magnesia, a substance well known to be slightly alkaline. The trial was attended with perfect success; the improvement by the subcarbonate of magnesia was greater than by any of the other alkaline bodies.

The carbonate of magnesia, I conceived, might act on the bread in two ways: *chemically* and *mechanically*. *Chemically*, by correcting its tendency to acidity;—*mechanically*, by improving its texture. I did not think it necessary to enter into an elaborate analysis of the bread, in order to ascertain how far my views were correct; yet my experiments, some of which I shall give in detail, have perhaps been sufficiently minute to throw some light on the inquiry.

Carbonate of magnesia, I found, improved the colour of bread made from new seconds flour, whilst it impaired the colour of bread from fine old and new flour. This circumstance favoured the idea of its chemical action; for, if it were passive, it was inconceivable that a substance so perfectly white could in the slightest degree injure the colour of the whitest flour. I made the following experiments on bread from new seconds flour of bad quality, containing the carbonate of magnesia in the quantity of from 30 to 40 grains to a pound of flour.

I burnt two ounces of the bread in a Hessian crucible, at a low heat, until it became like a cinder; the temperature was then raised to a dull red; and after twenty minutes the black mass was partially covered with a light white substance, which, on trial, readily dissolved in diluted sulphuric acid with effervescence, and gave a white precipitate with carbonate of ammonia. The heat was continued until all the carbonaceous matter had disappeared, and there remained at the bottom of the crucible a small quantity of a white earthy-like substance. It had a saline taste, partially dissolved in water, and readily with effervescence in sulphuric acid. It was principally common salt used in the bread, and carbonate of magnesia. The preceding experiments were repeated with similar results.

I crumbled two ounces of the soft of the bread, and put it into a pint of distilled water. After remaining about twenty-four hours, the fluid was passed through a filter. (It was of a little darker colour than the fluid furnished by bread made from new or old flour alone.) It had an agreeable taste, and did not affect the colour of litmus. When the fluid was treated with a solution of subcarbonate of ammonia, a light flocculent precipitate soon made its appearance; it was immediately redissolved with effervescence by a few drops of sulphuric acid.

When a half pint of the fluid was boiled down nearly to dryness, and gently heated with strong sulphuric acid, there was a

 violent

violent action, and fumes of acetic acid apparently mixed with a little muriatic acid were disengaged. The excess of sulphuric acid being expelled by heat, distilled water was added, and the solution filtered. On treating it with carbonate of ammonia, there was a copious light flocculent precipitate, which on further examination proved to be carbonate of magnesia. A further quantity of the original infusion of the bread was boiled down to dryness in a platinum crucible; it appeared to be principally mucilage. On being exposed to the atmosphere for a few hours, it slightly deliquesced; and when treated with strong sulphuric acid and carbonate of ammonia, the results were similar to those noticed above.

On two ounces of fresh bread a half pint of distilled water was poured. After remaining about twenty hours, the infusion was decanted, and boiled down to about 1-12th of its original volume. It was put into a tubulated retort with a little strong sulphuric acid, and a heat below the boiling point of the acid was applied for half an hour. The neck of the retort was placed in a glass containing a half cubic inch of pure water. A colourless fluid came over, which had a peculiar empyreumatic odour, a very slight acid taste, and reddened litmus paper. This fluid was neutralized by caustic potash, and evaporated to dryness. The dry mass was treated with a slight excess of strong sulphuric acid; and on being gently heated very pungent fumes, principally of acetic acid, were disengaged. An infusion of bread containing no carbonate of magnesia, on being treated with carbonate of ammonia gave no precipitate. A half pint of such an infusion after being evaporated to dryness, and heated to redness for some time, yielded a little white substance, which appeared to have undergone fusion. It was soluble in water, gave no precipitate with the carbonates of potash and ammonia, but a copious one with nitrate and nitrate of barytes, and was merely the common salt used in the bread.

The foregoing experiments appear to sanction the conclusion, that when the carbonate of magnesia is mixed in certain quantities with the new flour, the magnesia acts chemically on the bread in the act of baking. One portion of it is decomposed by the acetic acid formed: the other part remains in its original state. The disengagement of the carbonic acid gas from the decomposed carbonate, may perhaps tend to increase the lightness and porosity of the bread.

As the new flour appears to contain an excess of gluten, a very tenacious substance, (on account of a portion of the farina of the wheat having been converted into saccharine matter,) the undecompounded carbonate in the bread may exert a mechanical agency on the gluten in the dough; and by lessening its cohesive property,

property, it may improve the texture of the bread, in a manner somewhat analogous to the improvement of stiff clay soils by the mechanical agency of sand or gravel.

Bread made from the new flour with the addition of carbonate of magnesia is much lighter and more porous than when made without it. I have at different times made loaves with and without the magnesia, using equal weights of the materials, and I have always found the magnesian loaves of much larger size: And I may further add (*cæteris paribus*), not only is the relative bulk of the bread increased by the use of magnesia; its actual weight is likewise greater. Magnesia appears to give to the new flour a greater capacity for water. For example: seventeen ounces of new seconds, containing forty grains of magnesia, required eleven ounces of water at the temperature of 80° Fahr. to make it into dough; but ten ounces at the same heat were sufficient for sixteen ounces of the flour. There was also a proportional increase in the weight of the bread. The loaf with magnesia (after making the necessary allowances) weighed 200 grains more than the one without it. Carbonate of magnesia, by giving bread the power of fixing an additional quantity of volatile materials, seems to act not unlike chalks or marles, when applied on a sandstone or gravelly soil; they increase its power of absorbing and retaining moisture.

The facts I have stated must, I should think, tend to obviate any objection against the use of small quantities of magnesia in bread, from the fear of its accumulation in the system, on account of its supposed insolubility. As cold water readily dissolves the new magnesian salt formed in the bread, the fluids of the stomach, it may be presumed, will be much more effectual solvents of that substance.

There is one circumstance that deserves mention, and with the notice of which I shall close the present communication. I think I have ascertained the existence of the prussic acid in bread made from the new flour*. My opinion rests on the fact, that in the preceding experiments, when the strong sulphuric acid was added to infusions of the bread evaporated to dryness, or nearly so, the peculiar peach-blossom odour of the prussic acid became more or less perceptible on a gentle application of heat. I do not conceive the appearance of this acid can be referred to any changes effected in the vegetable matter by the agency of the sulphuric acid, as the prussic odour was clearly perceived in cases when the sulphuric acid was in a very diluted state.

* From an experiment I made with bread made from good old flour, I am inclined to believe it also contained the prussic acid, but in much smaller quantity than the bread from new flour.

 Should

Should further experiments confirm the existence of the prussic acid in bread made from the new flour, the fact would in some measure serve to account for the injurious effects that have been attributed to it, especially when freely used by children.

Cork, Jan. 20, 1817. EDMUND DAVY.

XLI. *On alloying Iron with Manganese.* By DAVID MUSHET, *Esq. of Coleford, Forest of Dean.*

To Mr. Tilloch.

DEAR SIR,—THE general result of my two last communications showed that there existed a difficulty in combining to any considerable amount the metal of manganese with that of iron, either by the fusion of cast-iron with the ore of manganese, or by the fusion of the ores of both metals. Nothing favourable to a practical result in the blast-furnace could be inferred, particularly if it were necessary to unite manganese to the extent of 20 or 30 per cent. with the metal of iron for any particular object of manufacture. It was however remarked, that while the metallic results or buttons were obedient to the magnet, some minute spherules of metal were obtained, over which it had no influence. I therefore concluded that the difficulty arose from a defective mode of operation, and that those circumstances necessary to produce de-oxidation and reduction in ores of iron were not sufficient to produce a similar effect when an ore of manganese was operated upon. In the blast-furnace as well as in the assay-furnace previous de-oxidation is necessary to metallic reduction. In the former this is completely effected by a process of cementation which takes place in the upper regions of the furnace. In the latter, the fusion being more rapid, the effect is generally produced by the presence of a quantity of carbonaceous matter in the mixture. If the crucible is formed of a mixture of clay and this latter substance, the fusion will admit of a more rapid progress, and the oxidation and reduction will be more completely effected. Hence in the reduction of ores of iron in crucibles formed of clay and blacklead, metallic masses of iron may be obtained so highly saturated with carburet of iron as to destroy metallic weight and compactness: under such circumstances the most perfect de-oxidation takes place; no vestige of iron remains unrevived, or the slightest trace of its oxide in the glass. Considering the use of such crucibles favourable to the reduction of manganese, I determined on attempting the alloy of this metal with iron to a greater extent than

than I had hitherto found practicable. I made use of the same ores formerly mentioned: the fluxes were chalk and window-glass; the latter I preferred to an argillaceous earth, which, so far as it regards reduction, answers equally well, but does not exhibit so transparent a glass when the results are perfect or approaching thereto.

1st Exp. Iron ore	.. ..	..	..	..	500	grs.
Chalk	.. ..	..	..	..	400	
Window glass	..	..	..	..	500	
Charcoal	.. ..	..	..	..	160	

This mixture was perfectly reduced, though the glass indicated by its depth of green a portion of oxide still unrevived. The metallic button was smooth, its fracture and quality those of the finest or most carbonated cast-iron: weight 30 grains, equal to $47\frac{1}{2}$ per cent. The same experiment repeated under a less rapid fusion yielded 50 per cent. of iron. When equal quantities of chalk and glass were used, a carburetted metallic button was obtained equal to $50\frac{1}{2}$ per cent.

2d Exp. Iron ore	..	..	..	..	500	grs.
Chalk ..	..	..	..	..	400	
Glass ..	..	..	..	..	500	
Charcoal	..	..	..	..	160	
Oxide of manganese		..	..	..	100	

A perfect fusion of this mixture yielded a fine smooth carburetted button of iron, weighing 258 grains, equal to $51\frac{6}{10}$ per cent. The glass was perfect, of a lead-blue transparency, slightly tinged with the purple peculiar to manganese.

3d Exp. Iron ore	..	..	..	..	500	grs.
Chalk ..	..	..	..	..	400	
Glass ..	..	..	..	..	500	
Charcoal	..	..	..	..	160	
Manganese roasted		..	..	..	100	

Though in this experiment the manganese had by previous roasting been deprived of 20 per cent. of oxygen and moisture, yet no perceptible difference took place in the result. The metal and glass were similar. The weight was 260 grains, or 52 per cent. The metallic results in both were magnetic.

4th Exp. Iron-stone	..	..	..	..	500	grs.
Roasted manganese		..	..	..	200	
Chalk ..	..	..	..	..	400	
Glass ..	..	..	..	..	500	
Charcoal	..	..	..	..	160	

The fusion of this mixture was very complete; the surface a perfect transparent pale glass covered with a coating of carburet of iron. There were besides about 40 grains resembling

 the

the keesh of No. 1. pig-iron unacted upon. The metallic button was covered with a splendid coating of plumbago of the colour of silver. The globules were equally carburetted, but still magnetic. The whole weighed 276 grains, which is equal to $55\frac{4}{10}$ per cent. If the produce of the ore is taken at 50, which is near truth, then an alloy of metallic manganese of nearly $5\frac{1}{2}$ parts with 50 of iron was formed—a higher combination than any hitherto obtained in these experiments: the increase of metallic weight beyond a produce of 50 per cent. is 26 grains. This being obtained from 200 grains of manganese, is a produce from this ore of only 13 per cent. A slight diminution of magnetic influence was perceptible in part of the metallic result of this experiment.

5th Exp.	Iron ore	500 grs.
	Roasted ore of manganese	300
	Chalk	400
	Glass	500
	Charcoal	160

This mixture was also perfectly fused. The surface of the glass was not, as in No. 4, covered with shining carburet, but with a rough cream-coloured porcelain minutely punctured and in some instances crystallized. Beneath, a transparent smoky-coloured glass was found, very different from common assay-glasses of iron. The metallic button and globules were covered with the same coating of plumbago described in the last experiment. Weight 290 grains, or 58 per cent. Increase beyond the product of the iron ore 40 grains, which from 300 grains of ore of manganese is $13\frac{1}{3}$ per cent. Gained over last experiment only $\frac{1}{3}$ per cent.

6th Exp.	Iron ore	500 grs.
	Roasted ore of manganese	400
	Chalk	400
	Glass	500
	Charcoal	160

From the fusion of this mixture a singularly composed glass appeared: the surface was stony, of a whitish colour, covering a stratum of similarly coloured porcelain. Beneath a fine transparent glass interspersed with circular concretions of crystallized porcelain, only two grains of shining carburet were found unacted upon. The metallic button and globules were equally shining and carburetted with the former experiments, and weighed 318 grains; which is a produce of $63\frac{6}{10}$ per cent. from the iron ore; which being a gain of 68 grains of metal beyond the assay produce of the iron ore, makes the yield of 400 grains of manganese equal to 17 per cent. This metallic alloy will therefore be

be composed of Iron	78·6	pts.
Metallic manganese	21·4	
	100	

The metallic button and some of the largest globules were not acted upon by the magnet.

7th Exp.	Iron ore	500	grs.
	Roasted ore of manganese	500	
	Chalk	400	
	Glass	500	
	Charcoal	160	

The fusion of this mixture afforded a carburetted metallic button equally keeshy and shining with the former.

Weight	254	grs.
Globules	70	
	324	

Equal to $64\frac{8}{10}$ per cent. This is an increase of metallic produce beyond that of the assay of 74 grains, which from 500 grains of ore of manganese is equal to $14\frac{8}{10}$ per cent. One hundred parts of this alloy will be composed of

Iron	78·6	pts.
Manganese	21·4	
	100	

The glass obtained in this as in the former experiment was of three kinds, though considerably different; the upper surface was glazed with a brown metallic coating mixed with carburet of iron; under this was a greenish-coloured porcelain with a few specks of more perfect glass. Beneath these was found the principal mass of glass, of a white agate colour, but not transparent. The smallest globule now obtained was not in the least attracted by the magnet.

8th Exp.	Iron ore	500	grs.
	Roasted ore of manganese	600	
	Chalk	400	
	Glass	500	
	Charcoal	160	

This mixture produced a button and globules still more highly carburetted than any formerly obtained, weighing 350 grains, equal to 70 per cent. produce from the ore of iron. This increase of metallic weight beyond the assay produce being 100 grains, is equal to $16\frac{6}{10}$ per cent. from the quantity of manganese employed. No part of the product was magnetic, though eight grains of magnetic carburet was found unacted upon.

The

The alloy now obtained will be composed of iron		71·4 pts.
Metallic maganese	..	28·6
		100

In about ten minutes after the crucible containing the above mixture was withdrawn from the furnace, the contents were turned out in a solid conical form, of a brownish copper colour; the metallic button occupying its proper station at the smaller end of the cone. The mass as it cooled began to crack, swell, and disintegrate; its colour became lighter—and irregular crystallization appeared in the dismembered fragments. These forms, however, were of short duration; every particle even the most minute became subject to violent motion, which rapidly hurried on a state of the most perfect decomposition; the whole matter in a few minutes passing from a stony state to that of an impalpable white powder.

9th Exp.	Iron ore	..	..	..	..	500 grs.
	Roasted ore of manganese			..	..	700
	Chalk	..	..	..	..	400
	Glass	..	..	..	..	500
	Charcoal	..	..	..	..	160

On withdrawing this crucible from the furnace and removing the cover its contents were found in a state of the most perfect fluidity. The cover was restored, and I went to call a friend to witness the expected decomposition. On our return to the laboratory (within five minutes from the time the crucible was taken from the furnace) we found it burst into pieces, and the whole contents heaped up in a state of fine powder, which as it cooled exhibited several metallic shades. A metallic button with a rough surface, but highly carburetted, was found in the powdery matter: this with some globules weighed 320 grains, so that a considerable diminution of produce was here experienced; from which it was inferred that the alloy of manganese with iron had in the last experiment reached its maximum under the circumstances and proportion of mixtures now used.

I was now satisfied that ores of manganese might be smelted with success along with our common argillaceous ironstone in the blast-furnace, with a considerable augmentation of metallic produce, and much pleased to have discovered the fact that iron alloyed with manganese in certain proportions ceases to be obedient to the magnet. This fact alone renders it extremely probable that the presence of manganese is not essential to the formation of good steel, and that those irons analysed by Bergman contain no notable quantity, seeing that the strongest and most durable magnets are made of steel manufactured from such iron.

Observations

Observations upon a minute Examination of the Results of the nine foregoing Experiments, relating to Fracture and magnetic Attraction.

No. 1. In which no manganese was used, possessed the fracture of No. 1. Pig iron, soft and capable of being easily filed and fully obedient to the magnet.

Nos. 2 and 3. As relates to fracture, were fully more carbonated than the former; and though 100 grains of manganese had been fused in the mixture, the quality of the iron was soft and easily filed. No diminution of magnetic force was perceptible.

No. 4. The fracture of this latter was materially different from the three former. The grain or crystal was smaller, the colour lighter, and considerably harder under the file; the globules were attracted by the magnet, but they approached with less force, and perceptibly indicated a diminution of this action.

No. 5. The present fracture as to grain was similar to the last, with the exception of two distinct white lines running parallel with the under surface of the metallic button. Magnetic force considerably diminished.

No. 6. The fracture of this result was regularly mottled on a white ground; the file had little or no impression; a magnet of the power of 20 pounds did not lift a globule of 180 grains.

No. 7. Fracture similar to No. 6, with the exception of an edging of gray minutely crystallized : the smallest globules were not in the least acted upon.

No. 8 and 9. Presented white silvery fractures, and so extremely brittle as to pound readily in an iron mortar, which indicates a quality very different from white fractured cast-iron. No part of the metallic results of these experiments was in the least acted upon by the magnet. One striking anomaly appeared throughout: The alloyed buttons after the addition of the ore of manganese presented surfaces covered with the most shining carburet of iron common at iron-foundries; and this effect seemed rather to increase as the quantity of manganese was enlarged and the carbonation of the fracture decreased,—a circumstance totally unknown in the fusion of simple ores of iron.

Dear sir, yours truly,

Coleford, Jan. 16, 1817. DAVID MUSHET.

P. S. Your last number contained an interesting account of a mass of native iron found in South America, which I suppose is the same alluded to in chemical and mineralogical books within the last twenty years. On reading the account, I was peculiarly struck with its resemblance to those masses of iron sometimes formed in the bottoms of our blast-furnaces in this country. Many

Many of these are nearly metallic, and weigh from ten to thirty thousand pounds. From such a mass I have lately seen specimens perfectly similar (judging from description) to those obtained from the American mass of native iron, and which were capable of being extended under the hammer. The circumstance of the Brazilian mass being found where the surrounding country is covered with a substance resembling an ore of iron is extremely curious. Those who have a difficulty in believing the descent of such mighty masses of matter from the atmosphere, might find pleasure by inquiring as to the probability of these metallic accumulations being the products of ancient metallurgical operations; and whether the plentiful diffusion of iron ore in their immediate neighbourhood is not the scoria surcharged with iron that had been evolved in the progress of these, perhaps antediluvian, manipulations.

XLII. *Method of preparing Lime, so as to preserve it for a long Time always ready for Mortar or Whitewash.*

To Mr. Tilloch.

Sir, — Observing with pleasure the readiness with which you insert in your useful Miscellany any information that may be useful, I send you the process employed in this part of the world for preserving lime. Those of your readers who have occasion to use lime must know, that when suffered to slack by the moisture of the atmosphere it at the same time imbibes carbonic acid, and after a time returns to the state of unburnt lime. If made up into mortar to be kept in a heap, it undergoes, though more slowly, the same change; and when not quite destroyed is found to have lost much of its binding quality, by the mixture of the outer *dead* part of the mass with the lime in the interior of the mass. All those evils may be avoided by the German process, which I doubt not will prove acceptable to many of your readers. I am, &c.

Fr. Vaik, Jan. 6, 1817. T. E.

The Process.

Take the lime as soon as possible after it is burnt; dig a pit in the earth about seven or eight feet long and four or five feet broad:—by this pit set a wooden trough, about six feet long, two feet broad, and one foot deep. At one end of this trough cut a hole about six inches square, before which nail a grating of iron with the bars about a quarter of an inch asunder. Inside the trough

trough have a slider to cover the grating, which can occasionally be drawn up. Put two or three bushels of lime at a time into the trough; throw plenty of water on it, and mix it well up with a large hoe perforated with holes. When there is a good quantity of liquid, draw up the slider, and let it run into the pit. The trough should have a small inclination, and hang six or eight inches over the pit. Throw more water on the remaining limestones, and those which will not slack are not sufficiently burnt, and may be returned to the kiln.

After lime, thus slacked, has been a few hours in the pit, it will take the consistence of paste, which it will preserve for years. It should be kept covered, to keep it clean and exclude the fixed air floating in the atmosphere, as well as to preserve it from heat or cold, especially the latter, as frost destroys it. For those who use larger quantities several pits should be dug, in preference to larger ones;—for smaller quantities a lesser pit may be used.

This lime mixed with water is very superior to whiting for whitewash, and, as it requires no size, is much more wholesome, as the nauseous effluvia from size, which always attracts damp, cause an insupportable smell. It gives a resplendent white for ceilings, and has a peculiar tenacity on walls, and in situations exposed to wet. It has the advantage of always being ready for use; for when mixed with a little water and a proper proportion of sand, mortar is prepared in a few minutes. According to my own experience, this lime is equally good after lying several years in the pit: and any one acquainted with this method of treating lime, and the process usual in England in preparing mortar, will not hesitate in giving this the decided preference.

XLIII. *Observations on the Changes produced in the new Wheat; and on the Means of improving the Flour.* By Edmund Davy, *Esq. Professor of Chemistry and Secretary to the Cork Institution.*

To Mr. Tilloch.

Dear Sir,—The subject on which I recently addressed you* has since occupied my attention. I have extended my researches, and selected from previous inquiries such observations and experiments as were unnoticed in my late communication.

It appears to be an indisputable fact, that the average quality of the last year's wheat is very inferior to that of former years. Whatever defects it may possess, they must be referred to changes produced in the grain in consequence of the unfavourable cir-

* See p. 161.

cumstances

cumstances under which the crops ripened or were saved in the late unpropitious harvest. Every attempt, therefore, to improve the new flour for the purpose of making bread, ought to be founded on some previous knowledge of the injury the wheat had sustained. The hope of being able to correct the bad qualities of the flour, induced me more than two months ago to turn my leisure moments to the subject. I entered on the investigation with some preliminary experiments on the new wheat and flour. I examined several samples of new wheat: they had all to a certain extent undergone the process of germination or incipient vegetation. In some cases the germen had protruded from about 1-10th to 5-10ths of an inch, and the radicle from about 1-10th to above an inch in length. These were, it is true, the very worst samples I could procure. I likewise examined several specimens of wheat from different parts of this county, of a quality far superior to those I have mentioned; but in all of them it was discoverable on a minute inspection that a slight and partial germination had taken place. And I have been given to understand, that the average wheat of the country has suffered more or less in this way. Some crops germinated before they were ready for the sickle, and others after they were cut down and made up into sheaves.

As the natural germination of the wheat appeared to be similar to that effected by artificial means, in the malting of barley, in which a portion of the farinaceous part of the grain is converted into saccharine matter, I made some experiments with a view to ascertain how far this opinion was well founded. I placed different samples of new wheat in tin dishes, and thoroughly dried them on a hot iron plate. The grain had a sweetish taste resembling malt, and afforded with warm water an infusion which when boiled down nearly to dryness had a sweet taste, and consisted of saccharine and mucilaginous matter.

I mixed some new seconds flour of bad quality with distilled water. After about forty hours the fluid was passed through a filter. The solution had a slight sweetish taste; on being boiled a little albumen coagulated, and after the fluid was evaporated to 1-14th of its original volume, the residual liquor was quite sweet to the taste, and consisted of mucilage and sugar. On making a comparative trial with good old flour, the quantity of saccharine matter obtained was, in proportion, very inconsiderable.

I made bread with the new seconds flour, warm water, and yeast, without any salt. Some of the bread was put in distilled water for twenty-four hours; the clear fluid was then decanted and boiled down nearly to dryness. The residual liquor was sweet. It was further evaporated and digested in warm alcohol. The

The sugar was dissolved by the spirit, and there remained a white substance which dissolved in cold water, and was principally mucilage. The spirituous solution was boiled to dryness, and the sugar remained. It tasted of the spirit, and was also mixed with a little mucilage.

From these trials it seemed probable that the bad qualities of the new flour were connected with the production of saccharine matter in the grain; and this opinion acquired additional evidence from the following experiments.

I made two small loaves; each contained half a pound of the best old flour and the same quantity of yeast and salt. In fact, they were in every respect similar, with the exception that one contained 100 grains of soft sugar. Before they were baked, it was observed that the dough with the sugar was not quite so light, it had not apparently fermented so well as the other. The loaves after being taken from the oven and suffered to cool were examined. The loaf without sugar appeared to have all the requisites of excellent bread. The property of elasticity it possessed in a high degree. When the soft part of the bread was pressed ever so much by the finger, it immediately regained its former situation without leaving any impression behind. The loaf with sugar had a bitter taste, and especially the crust of it. It was rather disposed to be clammy. It was not nearly so elastic as the bread without sugar. When pressed on by the finger it slowly recovered its situation, but the impression was left behind. The colour of the bread with the sugar was also a little impaired. I made a similar trial with good old American flour, and the results corresponded very nearly with those detailed; the bread with sugar had a bitter taste, and slightly adhered to the teeth; whilst the bread without sugar was excellent, and did not possess those properties in any degree. As an infusion (of the bread containing sugar) in cold water had a sweetish taste and did not affect litmus paper, it is likely a part at least of the sugar remained unaltered in the bread.

From these experiments it would appear, that the quality of bread made from good flour, with the usual additions, is impaired by sugar; as the dough with sugar did not rise quite so well, nor was the bread so light as without sugar. I should conceive this substance tended rather to check than to accelerate fermentation in such circumstances. A certain portion of saccharine matter seems essential to the panary fermentation. Good wheat appears to possess the requisite quantity, which, if it be increased by artificial or natural means—whether it be simply added to the flour, or produced by the germination of the grain,—still it seems to act by retarding the fermentation requisite to produce good bread, or bread in its greatest perfection. The case

is

is somewhat similar with preserved fruits beginning to undergo the acetous fermentation; when they are heated with more sugar, this process is checked, and all further chemical changes are suspended. Common salt and other saline substances, as is well known, are powerful antiseptics when used in quantity, but when employed in small proportion they promote putrefaction. Sugar in a similar way appears to retard or promote fermentation.

The efficacy of the method recommended by Mr. Hatchett of improving damaged and musty corn, by exposing it to the agency of hot water, may in part depend upon its dissolving out a portion of the saccharine as well as the musty matter of the grain.

The means recommended to improve the new flour for making bread, are 1. By drying the flour. 2. By mixing with the flour or the dough some innocent and cheap material. The flour may be dried either *slowly*, by exposing a large surface of it before the fire for several hours, and occasionally turning it over; or *rapidly* in an oven, or by the agency of steam. With a view to ascertain the most efficient mode of drying the flour. I made the following experiments on new seconds of bad quality.

Exp. 1.—One pound of flour in a shallow tin dish was placed before the fire, and occasionally turned over. After twenty-four hours it lost one ounce in weight. The bread from it was of a better colour, lighter, and rather better-tasted than the flour would have furnished before it was dried. Still, however, it had a disagreeable taste, which was effectually corrected by the carbonate of magnesia, when used in the proportion of thirty grains to a pound of the flour.

Exp. 2.—One pound of flour in a tin dish was placed on an iron plate for eight hours, at a temperature varying from about 200° to 230°. It lost two ounces in weight. The bread from it was not improved, but heavy, clammy, and of a bad colour.

Exp. 3.—One pound of flour in a dish was put into the oven and exposed to a temperature varying from about 212° to 250° for three hours; it lost nearly an ounce in weight. The bread from it was very little better than that of the preceding experiment.

To ascertain whether the flour, by being slowly dried, lost any thing besides moisture, I put two ounces of it into a tubulated retort, placed the neck of the retort in a little distilled water, and exposed the retort to the temperature of about 90° for some time. A little fluid came over. It was colourless and insipid, and did not affect litmus or turmeric paper; and left no residuum on being evaporated to dryness. Hence it appeared to be only water.—These experiments seem to favour the opinion that the advantages resulting from drying the flour are best secured by carrying on this process in a slow and gradual manner. And as

the

the new flour in the act of drying appears to lose nothing but water, and after being well dried it still makes bread of indifferent quality, the process of drying can only be regarded as a useful auxiliary, and by no means supersedes the application of other substances as correctives. I have said a moderate warmth, long continued, seems to improve the flour more than a higher temperature. This difference may probably arise from various considerations. As sugar appears to be injurious to flour, and the new flour contains an excess of saccharine matter, and is in a comparatively moist state; a low temperature is most favourable to incipient fermentation, by which process the excess of sweet matter in the flour may be changed into vinous spirit and be dissipated in drying. My experiments have been too limited to enable me to speak accurately concerning the relations of the flour dried in different ways, to water. I obtained one result that was rather unexpected:—I shall state the experiment, but as I had not an opportunity of repeating it I shall decline all comment on it. I dried eleven ounces of bad new flour on a hot hearth; in six hours it had lost nearly two ounces. I made the dry flour up into dough, but found that it required less water for this purpose than an equal weight of the undried flour.

The principal substances employed to improve the new flour for bread appear to be the alkalies and their compounds, and the subcarbonate of magnesia; and I pressume they have been found more efficacious in this way, than any bodies that have yet been tried.

In my former communications I have recommended the use of the subcarbonate of magnesia, and endeavoured to point out its agency when employed in bread. I shall now briefly state some comparative experiments I made, which led me to recommend magnesia in preference to any of the other alkaline substances. I made fourteen small loaves of the new seconds flour of bad quality: each loaf contained half a pound of flour and nearly the same quantity of yeast, salt, and warm water.

No. 1. Loaf containing only the above substances; quite heavy and so clammy as readily to adhere to a knife and the fingers;—bad tasted.

2. Loaf with ten grains of subcarbonate of ammonia in solution;—Lighter and better tasted than No. 1. but rather clammy.

3. Loaf with twenty grains stick potash (*kali purum*);—Better than No. 1., but not so good as No. 2.

4. Loaf with twenty grains subcarbonate potash;—Pretty good, rather better than No. 2.

5. Loaf with ten grains American potash;—Improved, but not equal to No. 2.

No. 6. Loaf with eight grains pure potash;—Not so good as No. 5.
7. Loaf with eight grains pure soda;—Inferior to No. 6.
8. Loaf with twenty grains carbonate soda;—Just as bad as No. 1; quite viscous, adhering to the teeth.
9. Loaf with forty grains ditto;—Better than No. 8, but inferior to No. 2.
10. Loaf with ten grains pearlash;—Improved, but not materially.
11. Loaf twenty grains ditto;—Tolerably good, scarcely inferior to No. 2.
12. Loaf with little pure ammonia;—Scarcely improved, clammy, and bad tasted.
13. Loaf with twenty grains of subcarbonate of magnesia;—Very much improved; better than any of the preceding, light and porous, good tasted, and not in the least clammy.
14. Loaf fifteen grains ditto;—Scarcely inferior to No. 13.

When the new flour is well dried, and the subcarbonate magnesia mixed with it in the quantity of thirty grains to a pound of flour; good bread may be made from such flour with the addition of 1-8th or 1-6th of boiled parsnips, or of baked or boiled potatoes. Baked potatoes seem to answer better than boiled ones; they are drier, and more mealy.

Bread made from the new flour of indifferent or bad quality has a disagreeable smell and taste, is disposed to be heavy and clammy, and has its colour impaired. The disagreeable smell and taste of the bread seem to be connected with the presence of an excess of saccharine matter in the flour, and the subsequent changes it undergoes in the process of baking. The application of the alkaline bodies is explained on the idea that the bread has a tendency to acidity, which is counteracted by their presence; or they neutralize any acid formed, and thus materially improve the bread.

From a comparative trial I made, it would appear that the new flour contains a larger proportion of gluten, but less farina, than the old flour. As the gluten of wheat is a very tenacious and adhesive substance, and of a dull gray colour, and farina of a white colour; and as the former is in excess, and the latter in deficiency, in the new flour;—these circumstances may in some measure serve to explain why the bread is disposed to be clammy and of a darker colour than is usually the case.

I remain, dear sir, yours truly,

Cork, Jan. 27, 1817. EDMUND DAVY.

XLIV. *New*

XLIV. *New Theorems for determining the Rate of Interest, and the Value of increasing Annuities, &c. &c.*

To Mr. Tilloch.

Sir, — As you were pleased on a recent occasion to give a favourable reception to a paper of mine on the subject of increasing annuities, I am induced to communicate the present one, which may in a degree be considered as a continuation of the former. I shall first, however, give the necessary theorems for the amount and present value of an annuity increasing according to the numbers 1.3.6.10.15, or figurative numbers of the first order; and the succeeding two will be general expressions for the sums of the series, denoting the present value of annuities increasing in the manner stated in the latter part of the paper already referred to.

First, then, of an annuity increasing annually, by the numbers 1.3.6.10 ... $\frac{n^2+n}{2}$ the general theorem for its amount in n years, putting x for $1+r$. the amount of 1*l.* in a year, will be

$$\frac{\frac{2x}{x-1}.(x^{n+1}-1)-(\overline{x-1}.\overline{n^2+3n+2}+n+1)}{2(x-1)^2}$$

And that for the present value of the same annuity is

$$\frac{\frac{2x}{x-1}.(x-x^{-n})-(\overline{2n+1}.x^{-n}+x-1(n^2+3n+2).x^{-n})}{2(x-1)^2}.$$

Again, the expression for the present value of an annuity increasing in the ratio of the numbers 1.3.5.7.9.11 is

$$\frac{(x+1)-(2.x^{-n}+(x-1.2n+\overline{1}.x^{-n}))}{(x-1)^2};$$

and if it be supposed increasing by the squares of these numbers, the present value will be expressed by

$$\frac{\frac{8}{x-1}.(x-x^{-n})+x-1-8(n+1\,x^{-n})-x-1.x^{-n}.2n+1^2)}{(x-1)^2}.$$

In each of the three last theorems, if n is infinite, or the annuity is a perpetuity; then, as all the quantities affected by n vanish, or become nothing, they will be reduced to these following:

$$\left(\frac{x^2}{(x-1)^3}\right).\left(\frac{x+1}{(x-1)^2}\right).\left(\frac{8x}{(x-1)^3}\right).$$

Which therefore are general expressions for the value of the perpetuities under the above circumstances respectively.

As

As the operation of constructing tables of the amount and present value of one pound annuity for any given term is not very commodiously performed, according to the common method, I shall in this place show how it may be effected in a much easier manner.

Let A.A′ denote the amount of an annuity for n and $n+1$ years respectively; then A equal $\frac{A'-1}{1+r}$, and hence $A' = (A \times \overline{1+r} + 1)$.

If, again, the same letters be made to denote the present value of an annuity of 1*l.* per annum, we shall similarly have $A' \times \overline{1+r} - 1 = A$ and $A' = \frac{A+1}{1+r}$.

In this way may we examine and extend the tables at pleasure; and the amount or present value of 1*l.* corresponding to the like or any other term of years, will then be obtained by simple subtraction only: and thus a process otherwise tedious is superseded.

I come now to the determination of the rate of interest in annuities; a subject that has engaged the attention of some of the greatest mathematicians that ever lived. Various formulæ have been devised for this purpose, most of which give the value sufficiently exact, when the number of years is not very great; but in this latter case no dependence can be placed upon the result, and at times the formulæ wholly fail. Those which I have to propose are the following; to which is annexed an example in each case, to show their degree of accuracy.

But I must first premise that S and P signify the amount and present value of 1*l.* annuity for n years $\left(\frac{S}{n}\right)^{\frac{2}{n-1}} - 1 = Z$ $\left(\frac{n}{P}\right)^{\frac{2}{n+1}} - 1 = Z'$ R.R′ being two assumed rates, deduced from a comparison of S and P with the values nearest them in the tables, R′ being less and R greater, or both less, or greater, than the true rate r.

Then in either case we shall have

$$r = \frac{12Z + (\overline{n+1}.R)R}{12 + \overline{n+1}.(R+R')}, \quad r = \frac{12Z' - (\overline{n-1}.R)R'}{12 - \overline{n-1}.(R+R')}.$$

An annuity of one pound per annum in fifty years amounts to 209,348*l.*—what is the rate of interest allowed?

Here we see by the tables that the rate is exactly ·05 or 5 per cent. But let us suppose it either ·0475 or ·052: hence putting $\left(\frac{209.348}{50}\right)^{\frac{2}{49}} - 1 = \cdot 06019$; we have by the first formula

0475

0475	·06019	052	
052	12	052	
·0995	72228	·002704	05037
51		51	05
5·0745		·137904	·00037
12		72228	
17·0745			

17·0745 ÷ ·860184 (= ·05037 which exceeds the true rate or 5 per cent. by the $\frac{37}{100000}$ part of itself.

The present value of an annuity of 1*l.* for fifty years is 18,25592*l.*—what is the rate of interest?

In this case also we see that the rate is exactly ·05; but let it be supposed, as before, either ·0475 or ·052: then putting $Z' =$ ·0403, and making use of the second formula, we find

0403	·0995	4836	052
12	49	12103	0475
·4836	4·8755	7·1245 ÷ 36257 (·0507	·00247
	12		49
	7·1245		·12103

Here the difference is $\frac{7}{10000}$ more than the true rate.

In these two instances, selected promiscuously, it will be seen that the formula is rather erroneous in point of excess; and this in general will be the case when there is much difference between the assumed rates: hence, under such circumstances, if the rate appears to consist of an integer and some even fractional quantity, we may properly take such rate, and fractional part or that nearest it, for the exact rate per cent., rejecting all the succeeding decimal places.

But this is not necessary; since from the tables we can always deduce approximate rates nearer than those above, and the value of r thereby will in every case be rendered extremely correct.

The rate of interest may also be pretty correctly obtained by the aid of trigonometrical lines, sines, and tangents.

To do this let $m = \frac{6}{n+1}$ $v = \frac{6}{n-1}$, and assume $\frac{2Z}{m} =$ $\text{tang}^2\, O$ $\frac{2Z'}{v} = \text{sine}^2\, O$.

Then $\log \text{tang}^t\, O = \frac{1}{2}(20 + \log 2 + \log n + 1 + \log Z - \log 6)$

$$\log\ \text{sine}\ O = \frac{1}{2}(20 + \log 2 + \log n - 1 + \log Z' - \log 6)$$

And in case of an amount

$$\log r = (\log m + \log \text{tang}^t\, O + \log \text{tang}^t \frac{O}{2} - 20);$$

And in the present worth

$$\log r = (\log 2v + 2 \log \text{sine} \frac{O}{2} - 20).$$

In computations relating to subjects of this nature, it often becomes necessary to determine the time when a given sum at an assigned rate will double itself at compound interest; or, the time being known, to determine the ratio of the rate. In this case we derive from the equation $\frac{\log 2}{n} = \log 1 + r$, but r may be found without logarithms, from this expression

$$r = \frac{n - \overline{n(n - 1 - \cdot 386)}^{\frac{1}{2}}}{n}.$$

All the foregoing theorems will apply generally to the several objects detailed. They are here submitted with a view that others may avail themselves of any advantage they possess, or facility they may afford in the resolution of particular cases: but the investigations have been reserved for another performance which I have at present in contemplation.

Haberdashers Place, Hoxton, March 6, 1817. JAMES B. BENWELL.

XLV. *Some Account of the Solar Spots which appeared during the Year* 1816*.

THE following observations were made in consequence of a report very generally circulated during the summer of 1816, that the solar spots were, at that time, more numerous than usual, and of extraordinary size. Many persons connecting this circumstance with the state of the atmosphere, conceived that the wet and cold season, which proved so injurious to the harvest, was in a great measure caused by the interruption of the sun's rays by these opaque parts, which were supposed to occupy an extensive portion of its disk. Such an opinion is at first sight sufficiently plausible to attract the attention of those unaccustomed to observe the appearances presented by the solar maculæ; and some statements which were made in the Philosophical Transactions of London for the year 1801 have long since tended to give this opinion universal publicity. Not, indeed, from any important facts which were then newly disclosed, nor from reasoning founded upon them; but from an amusing conclusion which was drawn:—"that the price of wheat in Windsor market was in some degree connected with the spots in the sun." The humour of this result spread abroad by the newspapers, precluded all reasoning; and although the truly profound astronomer speedily retracted his theory, it was too late to recall the whimsical antithesis which had already caused the wise and the ignorant alike to smile†.

In

* For these observations we are indebted to W. M. Moseley, Esq. of Winterdyne-House, Worcestershire.

† The writer in stating this circumstance has no wish, by any means, to revive,

In regard to the spots which appeared on the sun during the last year (1816) it may be remarked, that two which passed over the disk in September were the largest which occurred. Their situation and form are given in No. 1, (Plate III.) as they were seen on the 16th of the above month. They were each surrounded by an umbra, and preserved nearly the same relative position, with respect to each other, during their progress over the disk; their course being parallel with and near to the sun's equator. The larger of the two occupied about 1-25th part of the sun's diameter.

As these maculæ were very distinctly marked with an opaque centre, encompassed by an umbra and of considerable magnitude, they afforded a good opportunity of comparing their appearance with the theory about forty years since advanced by Dr. Wilson the astronomer of Glasgow. He conceived these maculæ to consist of vast cavities in the substance of the sun; that the dark nucleus in the middle was the bottom or deep part of the cavity; and that the nebulous circle or umbra was produced by light faintly reflected from the sloping sides for some depth below the orifice. In support of this theory the Doctor observed, that when a spot accompanied by an umbra entered the limb of the sun, the dark nucleus always appeared close to that side of the umbra which was nearest the sun's centre; but as the spot advanced towards the middle of the disk, the breadth of the umbra gradually enlarged, till the nucleus seemed uniformly surrounded by it. Having passed the centre, the breadth of the umbra began to contract, on that side next the middle of the sun, till it reached the opposite limb, in which situation the nucleus seemed to touch the edge of the umbra as before, presenting through its course, in a reversed order, the same gradation exhibited while advancing towards the centre.

The appearances, however, which attended the spots in September last, did not correspond with the phænomena observed by Dr. Wilson. The writer of this, bearing in mind the above theory, could not discover that the nucleus ever touched the edge of the umbra; nor did the nebulous circle contract, as the spots receded from the one limb, or approached the other in their passage, further than might be attributed to the oblique position of objects placed on the surface. The cloudy weather prevented any observation being taken till the spots had arrived at a quarter of the sun's diameter, at which time the nucleus of

revive, invidiously, an old though harmless joke. He designs only to show that the opinions entertained by great philosophers often make deep and lasting impressions on the public, whether of serious or light tendency: for no other reason than that such opinions have been promulgated by men eminently great.

 each

each was uniformly surrounded by its umbra. On the 16th, however, an opportunity occurred for examining them when very near the western limb, and their figure is delineated in No. 1. Unfortunately, on the 17th the sky was overcast, otherwise the leading spot would have been seen passing the limb, when in all probability an indenture or notch would have been perceived. On the same day, too, the oblique position of the spots would have rendered it more easy to discover any alteration in the breadth of the umbra, beyond what the rectilineal view might have occasioned. Previous to the 16th, however, no change was visible, except only that in approaching the limb the spots seemed to become closer together than when near the centre. Further observations, therefore, appear necessary in order to establish Dr. Wilson's theory; which indeed, were the phænomena he describes uniform and constant, would be extremely rational and probable.

These spots did not reappear with the next revolution of the sun, as might have been expected, at the end of the month; nor did the disk of the sun present any thing remarkable till the middle of October; when, on the 16th, two small spots moving in a line were faintly seen near the centre. On the 17th, 18th and 20th, they appeared full to view; and having traversed the disk almost in a line, the foremost passed the western limb on the 21st, at an angle of about forty-three degrees south of the equator. In general the course of the maculæ is, with a little variation, parallel to the sun's equator; but the direction in which these proceeded was very singular and curious. If mechanical causes are sought to account for this peculiar motion, it must be concluded that the spots were impelled by two forces acting at right angles to each other. The revolution of the sun would give one power, in the direction of the equator; but what force could be exerted in a line with the polar axis appears inexplicable. See No. 2.

On November 1st, two spots were seen, the one a little south of the sun's equator, and not far from the line of its axis; and another somewhat larger in size, north of the equator, and rather nearer to the line of the poles. On the following day (Nov. 2d) the southern spot had advanced, but in an oblique direction; and that to the north had passed over a space *more than equal to one fourth* of the diameter of the disk, and was stationed about halfway between the centre and western limb. (See No. 3.) The nature of the sun is so totally unknown, that it is impossible to assign any philosophical reason for this accelerated motion; but it is obvious from this example, as well as from the circumstances noticed in October (see No. 2), as above related, that the spots are floating substances, not adhering to the surface of the sun; otherwise

otherwise they would not deviate far from the line pursued by the revolution of its axis. This indeed has long been ascertained to be the case; for though the general course in which the spots move, *usually* corresponds with the line of the sun's equator, they have sometimes been seen to pass in various directions within the equatorial region. Speaking of these maculæ, Adams says, "the paths they describe in their course over the disk are extremely different, sometimes being in straight lines, sometimes in curves; at one time descending from the northern to the southern part of the disk, at other times ascending from the southern to the northern part*." It must be observed, however, that these irregularities are of rare occurrence—that the spots very seldom pass what may be called the tropics of the sun, and that they have never been seen near the poles.

Few, if any, of the spots which appeared during the last year, are to be considered of large size; nor were there many surrounded with an umbra, which is usually the case with those of large dimensions. The two above described, which were seen in September, though larger than any others, were by no means to be esteemed as occupying any material portion of the disk; and although numerous small spots, sometimes scattered without order, and sometimes collected in clusters, were almost always visible; yet the quantity of light intercepted by them must have been perfectly insignificant compared with the whole area†. The spots also remained in general but a short time, scarcely ever could they be traced through two successive revolutions; and indeed none have ever been known to continue long. As an instance of the rapidity with which they sometimes change, a circumstance observed in October last will fully exemplify. On the 8th of that month, there were fourteen spots of various sizes upon the sun's disk; most of them faint and small, as figured and coloured *black*, in No. 4; the whole placed to the north of the equator: but on the 11th, the *black* spots were all entirely vanished, and three others were seen on the south of the equator, in the situation in which the spots are marked in outline. The intermediate days proved so cloudy, that no observation was made, whether this change had been produced suddenly or gradually.

With the exception of these appearances, nothing of the least importance occurred to the writer's notice during the last six months of the year 1816, with respect to the solar maculæ. The disk of the sun is scarcely ever free from spots, but they are generally small and of short continuance, of course they present nothing worthy of particular observation.

* Lectures, vol. iv. p. 229.

† Some have stated that the large spots containing nuclei, are often converted into brighter spots, which they termed *faculi*. The writer of this has never been so fortunate as to see this phænomenon occur.

It may be concluded, therefore, from the foregoing statement, that the spots on the sun could have had no effect in producing the inclement summer and autumn of 1816, for these reasons: first, because no spots appeared upon the disk of sufficient magnitude to intercept any material portion of light; and consequently the atmosphere could not have been influenced by such a circumstance. Secondly, that although numerous spots may have existed, their duration was always too short to produce any permanent or great effect; and therefore it is unreasonable to suppose the atmosphere could have been affected by such transitory appearances. Thirdly, it must be remembered that if any extraordinary influence can be conceived to result from the solar maculæ, it would not be confined to one quarter of the world rather than another; since the diurnal rotation of the earth would, in succession, expose all climates and countries to the same privation *.

Feb. 1, 1817.

* It may not be improper to state, that the above observations were made with a three-feet-and-half achromatic telescope, to the end of which was adapted a light frame resembling a three-legged stool. This frame was fixed upon the telescope by means of a hole through which the smaller end of it was accurately fitted, being lined with cloth that it might be made firm, without injury to the telescope. The legs of the frame expand, and at the distance of seven or eight inches a thin circular board was fixed to the ends of them parallel with the eye-glass of the telescope. Upon this board a paper was placed, having a circle drawn on it of the same diameter as that produced by the image of the sun's rays falling upon the board with the focus of the eye-glass properly adjusted. Across the circle were drawn three lines; one exactly perpendicular to the horizon; another inclining eight degrees westward, representing the axis of the sun; and a third at right angles to the axis representing the equator. When the apparatus was used, the window-shutter was closed, except one fold; and over that was placed a thick piece of cloth, having a hole in the middle with a kind of sleeve inserted into it. The telescope was passed through this sleeve, which reeved up close round the tube with a string, and the end then put out at the window and directed to the sun. When the image was perceived on the paper, a small weight suspended by a fine silk thread affixed to the upper leg of the frame, threw a shadow across the centre of the disk, and the perpendicular line drawn on the paper was then made exactly to correspond with it. Thus prepared, the situation and size of any spots might be marked on the paper with a pencil; and by accurately adjusting the shadow of the silk to the vertical line on the paper in successive observations the motion of the spots may be traced with accuracy. It is necessary, however, in making observations of some continuance, where several spots are to be marked, that an assistant should carefully turn the rackwork to correspond with the motion of the sun, otherwise the disk and the circle on the paper will not coincide. A little practice renders this easy.

Messrs. W. and S. Jones, opticians in Holborn, are acquainted with the apparatus used by the writer, which may be adapted to any achromatic telescope; and gentlemen, and even young ladies, may be enabled to amuse themselves with observing the solar phænomena, without trouble or difficulty, and also without hurting their eyes.

XLVI. *Improvement on the Sliding-Rule.* By SILVANUS BEVAN, *Esq.*

MOST of the following description of a little invention of mine, which seems likely to extend the uses of the sliding-rule, was prepared about nine months ago with a view to insertion in the Philosophical Magazine; but more important matters having then occasioned it to be neglected, I now offer it for that purpose. I call it my invention, because it is new to me, and to all my scientific friends to whom I have shown it; and because the advantages which it promises over the old construction would probably have prevented it from lying dormant if it had previously occurred to others. And I think I am warranted in this conclusion by the circumstance, that but a very short time had elapsed after its first promulgation, when so much of it as appeared to be adapted to the purposes of the Board of Excise was introduced into general practice under its authority.

The annexed drawing (Pl. III. fig. 5) shows that, instead of having, like the common sliding-rule, a fixed and a moveable line of numbers, each reaching from 1 to 10, and repeated to a second 10; mine has one line reaching from 1 to 10, and another reaching from about 3 to 10, and thence onward to 3·3; or more exactly, from the square root of 10, 3·163, to the repetition of the same number; one of these lines being inverted, or counting from right to left, whilst the other is placed in the usual manner. By this construction, without any diminution of its uses (as I shall presently show), the sliding-rule is reduced to one half its usual bulk, to the great increase of its portability and convenience; or, if its original length be retained, the size and accuracy of its divisions are doubled. But these are the least of its advantages: for while it performs the usual operations of multiplication, division, and proportion, with as much facility as the common sliding-rule, it also shows, on inspection, the square root and all the factors of a number given. For, by the construction of the line of numbers, the distance between the first and second terms of a given proportion equals the distance between the third and fourth; and by the inversion of one of the scales it happens, that if the second and third terms are made to coincide, the first and fourth will coincide also. Hence it follows, that in any given position of the slide, the products of coincident numbers are equal throughout; and if one of these numbers be unity, the correspondent number is the product of every contiguous pair, which are consequently its factors, and its root is shown by their equality. For example: Let it be required to multiply 16 by 4:—place those two factors together, as in the figure; and opposite to 1 will be found the product 64. Divide 64 by 2:—place

place 1 opposite to 64; and against the divisor 2 will stand the quotient 32. Given 4, 8, and 8, to find a fourth proportional :— place 8 opposite to 8; against 4 is found 16. To find the factors of 64 :—bring 1 to that number; in this position of the scale every factor will be found opposite to its reciprocal; as 2 against 32, 4 against 16, 8 against 8, which is consequently the square root of the given number. Or, if the factors of 640 be required, the same position of the slide shows that 2×320, 4×160, 5×128, 8×80, 10×64, 16×40, and 20×32, equally produce that number; whilst the juxtaposition of 25·3 in both lines shows that to be its root with as much accuracy as can be expected from so short a radius.

The scale reduced in length in the manner above described, but without the inversion of either of its lines, is competent to the solution of all questions of simple multiplication or division; and in this form it is prepared for the use of the officers of the Excise by R. B. Bate, of the Poultry, the accuracy and neatness of whose workmanship I cannot avoid mentioning, though I am sensible they are too well known to derive any publicity from this notice of them.

Feb. 8, 1817. SILVANUS BEVAN.

XLVII. *Some Remarks upon an urinary Calculus having a Cinder as its Nucleus; with a Notice of other Calculi containing a small Quantity of Carbonate of Lime. The occurrence also of Brown Spar, noticed in the Trap Rock near Newport in Gloucestershire.*

To Mr. Tilloch.

SIR, — HAVING seen in one of your late numbers an account of an urinary calculus chiefly composed of carbonate of lime, I have been induced to send you the following particulars of a calculus composed likewise of carbonate of lime, and further interesting from the substance forming its nucleus.

This substance is a common cinder, and it constitutes the greater part of the specimen. Although it is impossible to mistake its appearance, I may observe that sulph. acid when treated with a very small quantity of it very soon acquired a dark-brown colour. The cinder is surrounded but not intermixed with a layer of a yellowish-white colour, which dissolves in muriatic acid with a brisk effervescence, leaving behind a few tender flocculi. The solution being treated with subcarbonate of potash yielded carbonate of lime nearly to the amount of the original quantity employed; viz. two grains. To certify the presence of carbonic acid, I put a very small quantity of the calculus in a two-dram bottle, which immediately upon being filled with a dilute

lute acid and covered with a small bit of glass, was quickly inverted so as to stand upon the glass. In this way the bottle soon became filled with gas, which, upon being transferred into a small tube of lime-water and agitated in it, was quickly absorbed, rendering at the same time the lime-water milky. Hence the earthy part of the calculus is chiefly composed of carbonate of lime mixed with a little animal membrane. The exterior surface of the calculus is of a brown colour, and presents that kind of uneven appearance usually seen in mulberry calculi. The brown coating, however, is quite superficial, and is probably derived from animal matter, as I could find no trace of uric acid. Curious as the structure of this specimen is, I regret that the history of it is extremely obscure. It belongs to my friend Mr. Richard Smith senior, surgeon to the Bristol Infirmary, whose account of it I have transmitted to you.

Any one from its figure and appearance might consider it as a human calculus; but it is hardly possible to conceive how a cinder of this size could have been introduced into the bladder either of a male or female: besides which, the non-existence of uric acid which is generally present in some proportion or other in human calculi, and the fact of its being composed of carbonate of lime,—a very common ingredient in animal calculi, but a rare one in those of the human bladder,—are considerations very contradictory to this idea. May not the cinder have been wantonly thrust into the bladder of a cow or some other animal, and in this way have given origin to the present concretion?

I recollect Mr. Brande, in a paper of his in the Philosophical Transactions, speaks of several human urinary calculi having extraneous bodies for their nuclei, one of which was an hazel nut.

Having had free access to the extensive collection of Mr. Richard Smith, and having gone through the analysis of a variety of specimens, I may here take an opportunity of stating that I have met with three calculi, all human, which contained a small admixture of carbonate of lime. In dissolving a calculus (which I had reason to believe one of the ammoniaco-magnesian phosphates) in acetic acid, I was surprised at seeing a slow effervescence take place, which I found by the expedient above described to proceed from the disengagement of carbonic acid. The proportion of carbonate of lime was discovered by dissolving a few grains of it in dilute muriatic acid. To the solution pure ammonia was added, which threw down the phosphates of lime, ammonia and magnesia: these being separated, the solution was treated with carbonate of potash, and in another experiment with oxalate of ammonia, both of which gave a distinct precipitate.

The

The analysis of 11¾ grains of this calculus, conducted otherwise in the usual way, gave me

Of phosphate of magnesia and ammonia			..	5·0
Phosphate of lime	..	..	..	4·5
Carbonate of lime	..	..	..	·5
Uric acid, and loss	..	..	..	·55
				11·75

In another calculus principally composed of the ammoniaco-magnesian phosphate, the proportion of carbonate of lime was only half the above quantity; and in a third it was still less. I wish particularly to mention these instances of the occurrence of carbonate of lime, because Dr. Henry in his invaluable work (Elements of Chemistry) calls in question the authority of Proust, in giving carbonate of lime as an ingredient in urinary calculi.

I have seen somewhere a notice of Mr. Bakewell having discovered prehnite in the trap rock of Micklewood near Newport, Gloucestershire. I regret that the particular spot is not mentioned as I (among many others who have examined the hill) am extremely curious to know the exact locality. This rock is highly interesting, as it exhibits great variety in its structure; being sometimes a sort of iron clay, sometimes trap tuff, in another place exhibiting globular concretions of basaltic clinkstone; but the greater part of it is an amygdaloid consisting generally of a basis of iron clay, in which are oval concretions of green earth, and of a mineral of a white and brown colour resembling calc spar, and which has been by many mistaken as such. I have found the same substance in veins in the rock, and upon analysis I find that it is the mineral called *brown* spar, and contains 44 per cent. of carbonate of magnesia. I am, sir,

Very respectfully yours,

York Crescent, Clifton. W. H. Gilby, M.D.

*** Very well-executed drawings of the "cinder calculus" accompanied Dr. Gilby's communication; but being coloured, we cannot with convenience give an engraving of its figure, &c. In its longest diameter it is about two inches; in its shortest about one inch and a quarter. The drawings will be taken care of for Dr. G.

Mr. Smith's *Letter to Dr.* Gliby.

My Dear Sir,—The "cinder calculus" was given to me by Mr. B. G. Burroughs of Clifton, about a year since. Being in the habit of cutting them through, in order to exhibit the nucleus, I was not a little surprised to observe the saw in this case covered

covered with a black matter, and still more so when I found the strange substance of which it was chiefly composed. I became of course extremely anxious to learn its history. But this is all that I could obtain from Mr. B. "That it had been in his possession four or five years, and that the person who gave it to him told him that it was extracted by a Mr. Pye, and Mr. B. did not doubt that a human subject was implied."

Some gentlemen who have seen it imagine that it might have been taken from a watering-place in a street: but I have submitted substances to a trial of pouring urine upon them, and find the appearance entirely different. In the latter instances there is an even uniform stratum super stratum; whereas in this calculus there is a distinct tubercular appearance, resembling those denominated mulberry. Again: in the one case the layers are tender and easily brushed off, but here they are excessively firm.

One circumstance is not unworthy of remark, which is—that out of more than one hundred and fifty sets of calculi in my museum, this is the only one with the tubercular appearance which does *not* contain oxalic acid.

I remain, dear sir, yours truly,

38, Park Street. RICHARD SMITH.

To Dr. W. H. Gilly.

This calculus certainly contains no oxalate of lime, as it dissolves completely in dilute acetic acid; whereas oxalate of lime is insoluble in that acid.

XLVIII. *Abstract of "Instruction concerning the Making of Bread from damaged Corn. Framed by a special Commission named by His Excellency the Secretary of State for the Department of the Interior, and composed of Messrs.* GAU, *Honorary Counsellor of State, President;* MOREL DE VINDE, *Peer of France;* ST. MARTIN, *Commissary General of Hospitals;* BOSC, YVART, THENARD, GAY LUSSAC *and* SILVESTRE *(Secretary), Members of the Royal Academy of Sciences." Published* 28*th Feb.* 1817.

THE continual rains which have fallen this year during the months of July, August, and September*, have rendered the labours of the harvest difficult, and have affected a part of its produce.

* In the months of July and August 1816, about three times as much rain, and in the month of September about twice as much, fell, as in the corresponding months of the year 1815. The mean temperature for the nine first months of 1816 was two degrees less than that of the nine first months of 1815.

A crop

A crop which seemed to promise a remarkable fecundity, although it had been already a long time retarded by the influence of the cold and wet season, has occasioned much inquietude and fatigue to cultivators.

Some instructions have been published, in order to sustain their zeal and direct their labours. It has been recommended to them to put their sheaves under cover as soon as cut; and not to wait till the end of the harvest, to lead their produce home. It has been also advised, that when they are obliged to leave any sheaves upon the ground, they should form them into little upright stacks, put some covering over them, and thus secure them from the pernicious influence of the wet; and more particularly they have been cautioned against heaping wet sheaves in granaries or in stacks, and impressed with the necessity of thrashing them as soon as possible, in order to extract from them at least a part of the grain which they contain, and drying the grain before sending it to the mill.

These useful instructions have, without doubt, been sufficiently disseminated; but it is to be feared that an inconsiderate attachment to old habits has prevented them from being every where followed with that attention which they merit; and it becomes thus the more urgent to provide the proper remedy and prevent the progressive and prolonged increase of the evil.

Alterations in wet Grain.

Crops which have been for a long time more or less exposed to an abundant humidity, experience different sorts and different degrees of alterations. In each of these different states, they present different results—to the cultivator in regard to his seed—to the miller in grinding—and to the baker in bread-making.

Wet grain, when heaped up in granaries or in stacks, without currents of air being preserved through the interior, as has frequently been recommended, goes speedily to ruin; the humidity does not ascend to the top so as to evaporate; it concentrates in the interior; it rots the straw; and according to its abundance or its temperature, hastens the germination which has begun, or rather excites a fermentation which heats and discolours the grain; at times the grain becomes even mouldy, and the straw reduced to the state of dung.

The granaries and farm-yards have this year presented corn in all these different states. When the grain of such corn is sent without preparation to the mill, it clogs the millstones, and is difficult to work: if the germination has only just commenced, the process goes on and is soon completed in the sacks; and the flour made from it begins in a few days to collect into pieces of such a consistency, that it is necessary, in order to make

make it workable to pound it with mallets. Flour of this description is difficult to work, even when very speedily used; and when it gets old it is impossible to make bread of it without mixing it with some of a better sort.

Grain thus deteriorated always loses part of its natural weight. Thus while the good grain of 1815 and 1816 weighs commonly from 73 to 75 kilogrammes each hectolitre, the same quantity, if it has begun to germinate, will weigh only 61 kilogrammes 5 hectogrammes;—if greatly germinated, 55 kilogrammes;—if red-heated 63 kilogrammes;—and if moulded without being germinated, 57 kilogrammes. Nothing serves so well as weighing, to ascertain the degree of alteration which grain suffers from the effect of humidity.

The produce of grain in these different states is commonly as follows:—The good grain of 1815: 58 kilogrammes 5 hectogrammes of flour and pollard, 11 kilogrammes of bran. That of 1816: 56 kilogrammes 5 hectogrammes of flour and pollard, 14 kilogrammes 5 hectogrammes of bran. Grain slightly germinated: 40 kilogrammes 5 hectogrammes flour and pollard, 17 kilogrammes 2 hectogrammes 7 decagrammes bran. Greatly germinated: 36 kilogrammes 7 hectogrammes 5 decagrammes of flour and pollard, and 17 kilogrammes 7 hectogrammes 5 decagrammes of bran. Red-heated: 44 kilogrammes of flour and pollard, 15 kilogrammes of bran. And lastly, grain moulded produces only 35 kilogrammes 5 hectogrammes flour and pollard, and 13 kilogrammes 1 hectogramme of bran.

Damaged Grain ought not to be employed as Seed.

Grain which has germinated, been heated or moulded, ought by no means to be used as seed. In the experiments which have been made this year with much care, in order to determine in what degree grain which has been in different proportions deteriorated, dry and wet, may yet be available as seed, it has been found, that grain if it has suffered a commencement of germination does not rise but in the proportion of one half of the seed employed;—if strongly germinated, in the proportion of one-third: and if fired or moulded, of not more than one-fifth. The stalks are in all cases of a paler colour and of a less vigorous appearance than those from healthy seed, and give little promise of living to maturity.

Effects of Humidity on the constituent Parts of Corn.

It is chiefly the glutinous part which is altered in corn which has been exposed to humidity. The gluten loses almost entirely its adhesive powers; and dissolves into a sort of pap or starch, in place of presenting that consistency and elasticity which distinguish

stinguish flour of good quality, and are of such consequence in the making of bread.

Drying of Grain.

The drying of wet grain is the only means of arresting the progress of its destruction.

The most simple mode for this purpose, and that which can be most generally adopted, is to dry the grain in a baking-oven, which is to be met with in most country places.

The grain may be put into the oven immediately after the bread has been withdrawn; the temperature is then at such a degree that a person may introduce his naked arm without being much incommoded by the heat*. After the grain has been thrown into the oven, it should be spread into a bed of from eight to ten centimetres (from three to four inches) in thickness, and stirred frequently with a shovel or rake, in order to facilitate the disengagement of the vapour. At the end of ten or fifteen minutes, according to the state of humidity in which the grain is, it may be withdrawn from the oven: it will then be sufficiently dried; and when exposed to the air until perfectly cooled, will have acquired all the qualities necessary to render it fit both for the miller and the baker.

Ovens which are surmounted by a platform offer a very easy means of drying with more or less rapidity, according to the degree of temperature which it may be thought proper to give the platform.

Another very simple means which the Commission have employed, consists in drying the grain in a flat iron or copper cauldron. The dimensions of that employed were four feet in width, ten feet in length, and four inches in depth. At one of its extremities a stove was placed, of such a construction that either wood or pitcoal or turf might be burned in it; and from this stove the smoke was circulated under the cauldron by means of flues, which being made of brick served besides to support the cauldron itself. In adopting this method of drying, the bed of grain ought not to be more than one inch thick, and should be stirred from time to time with a rake. The temperature should be at least from 90 to 100 centigrades; perhaps it may even without inconvenience be raised as high as 130. The place in which the operation is carried on should be ventilated from time to time. One man may with an apparatus of this description dry more than twelve bushels of corn in an hour, reckoning

* In some of the experiments of the Commission the grain has been introduced into the oven when heated even to 190 degrees centigr. (150 degrees Reaumur). It has been then well dried in five minutes, and without suffering any injury.

that

that the corn does not contain more than seven per cent. of humidity *.

Process of Baking.

As the yeast is the principal agent in the fermentation, nothing is more important than that it should be procured in the best state. It ought to be such as has been very recently prepared, and on no account more than twenty-four hours old.

All potable waters are good for baking. The best flour imbibes about one half of its weight of water; middling good, from a fifth to a fourth.

The temperature of the water ought, in general, to be in an inverse ratio to that of the air—that is, as much colder as the air is hotter, and *vice versâ.*

The baking of flour which has been made from germinated grain ought to be proceeded in with much greater rapidity than that of flour from grain noways injured; because, the gluten of such flour having been more or less destroyed, the process of its fermentation goes on much quicker. The water employed ought to be of less warmth in all the operations; the paste should be kneaded more firmly, and divided into loaves of less thickness; the batch should be put into the oven a quarter or half of an hour sooner than usual, after it is completed: the oven should be raised to a higher temperature; the bread should be left in the oven only forty-five minutes or less, instead of an hour as in the ordinary case; and it ought not to be given out for consumption till two or three days after it has been baked. By attending to these directions, bread will be obtained from the flour of germinated corn, which, without being as good as that which is made from the best flour, will yet be sufficiently salubrious and of a good-enough appearance.

It is necessary to observe, however, that it is only from the flour of such corn as has been very slightly germinated that bread of the above description can be obtained, *unless the corn has been dried before being ground:* but when corn even greatly germinated has undergone such previous desiccation, it will yield a flour capable of making much better bread than flour from corn which, though less germinated, has not had the benefit of drying.

Previous drying has not, however, been found sufficient to render grain which has been fired or moulded capable of yielding an eatable bread, or removing the nauseous flavour and acrid taste which distinguishes grain thus deteriorated.

* In the numerous experiments which Duhamel has made upon the desiccation of wet or germinated corn, he has never found more than one-eighth of its weight in superabundant water; the mean quantity was only about a sixth.

Mixtures proper to ameliorate the making of Bread from the Flour of damaged Grain.

It is only by a mixture with good flour that the produce of damaged grain can be turned to good account. By joining only one-third of good flour to two-thirds of flour from fired or moulded grain, a bread will be obtained of a taste not unpalatable: but it is only by mixing a half or rather two-thirds of good flour with one-third of this inferior flour, that the taste of the bread produced is so improved as to entitle it to be considered as good household bread.

It deserves to be noticed that it is vain to employ a greater quantity of yeast in the hope of improving the fabrication of the bread. The paste deprived of gluten is unable to retain the effects of the fermentation excited by the yeast. The bread has a good enough external appearance; but in proportion as there has been an excess in the quantity of yeast, its consistency is so much the less, and all the bad qualities of the flour in respect of flavour and taste have a fuller development.

Substitutes for Wheaten Flour in the baking of Household Bread.

The mixture of a third of the flour of maize or barley or potatoes with a third of good flour, and a third of flour from heated or moulded grain, produces a bread fully equal to that fabricated by the mixture of two-thirds of good with one-third of damaged flour.

In the process of baking with such mixtures there is no difference from the ordinary mode: it is only necessary to observe that, in using maize or potatoes, the oven ought to be less heated than for barley or oats, and least so in the case of potatoes.

The following mixtures of these and other substances the Commission offer with confidence, as calculated to form excellent household bread.

Maize.

One-half maize and one-half barley, with a leaven of wheaten flour of one-fifth of the total weight.

One-half maize and one-half wheaten flour. A more agreeable and better bread it is impossible to eat.

Oats.

One-half oatmeal and one-half barley, with a leaven of wheaten flour of rather more than one-fifth.

One-half oatmeal and one-half wheaten flour. Excellent.

Barley.

Add to any quantity of barley-flour one-fifth of its weight in wheaten leaven. Bread white and savoury.

Barley

Barley and rye, or barley and wheat, in equal proportions. The last is equal to the best bread of wheaten flour alone.

Buck-wheat.

In equal proportions with barley or rye, and a wheaten leaven of one-fifth of the weight. Or still better with one entire half of wheaten flour.

Potatoes.

In general the potatoe may serve when it is dry for one-half, and when fresh or new for two-thirds, and even for four-fifths in the fabrication of household bread. This last quantity of four-fifths is the greatest which the Commission has been able to employ with advantage; but all their experiments with that proportion have been attended with uniform success.

Oats, barley, rice or maize, also mix well with potatoes, when used with a wheaten leaven of one-fifth of the total weight.

Employment of foreign Substances.

Various foreign substances have been recommended for ameliorating the fabrication of bread, and correcting the faults of damaged grain: such as the addition of alum, carbonate of soda, magnesia, diluted sulphuric acid, salt of tartar, vinegar, sulphate of iron, gums, &c. The Commission have made experiments with most of these substances; and some of them do appear, in fact, to possess in a slight degree the qualities which have been ascribed to them. But the Commission see no necessity for recurring to any such foreign aids,—most of which do not in themselves possess any nutritive quality,—as long as by a previous desiccation of the grain, by good grinding and by proper care, a wholesome bread may be obtained from all wet or germinated grain, and as long as, even in the case of flour of the most deteriorated description, it is only necessary to add a portion of good flour to obtain an excellent household bread.

XLIX. *On Aërial Navigation.* *By* A Correspondent.

It is a singular circumstance in the history of the arts, that an invention at its first appearance is frequently pursued with the greatest eagerness, and yet will afterwards be wholly neglected for years, until some happy improvement fixes it permanently on the public attention. Many will remember the great zeal excited by the subject of aërial navigation, and the astonishing subsequent neglect of an art so important, until the late revival of the subject by Sir George Cayley and other eminent philosophers.

 The

The following statement of every thing important which has been suggested on this point, with some new views, is offered for the purpose of facilitating further inquiries.

Vertical Motion.

1. The balloon being inflated with hydrogen, descends by letting out some of the gas, and ascends again by throwing out ballast. To this method it is a radical objection, that the means of alternate ascent and descent are very soon exhausted.

2. The air in the balloon being expanded by heat, the vertical motion is produced by increasing or diminishing the quantity of fuel. To this method it is an objection, that the fuel will ultimately be exhausted; also, if common air be used, the balloon must be of very large dimensions to support the car; and if the air be hydrogen, the expansion by heat is attended with the greatest danger.

3. The balloon being inflated with hydrogen, another is suspended below the car, and into this the circumjacent air is forced by an easy mechanical contrivance, and is let out again at pleasure. By these means the machine descends upon increasing the density of the air, and ascends upon restoring it to its former state. This method is worthy of peculiar consideration, not being liable to the former objections, and being analogous to that contrivance of Nature, by which fishes sink at pleasure, and rise again to the surface.

Lateral Motion.

1. The most obvious method of producing a lateral motion is by taking advantage of the winds. These are: occasional winds; trade winds between the tropics; the land and sea breezes which in warm climates set from and towards the shore by day and night alternately; the superior currents of air, which often proceed in a direction contrary to those below; and the breezes, which commonly follow the direction of every river. To these aids we may also add the remarkable phænomenon observed by all aërial navigators, viz. that the balloon sinks lower than usual when over water, and that it has a tendency to keep the direction of a river. This circumstance may partly be attributed to the wind following the current, but principally to the specific gravity of air impregnated with aqueous vapour being diminished, and the tendency of the machine to the point of least gravity.

2. The very ingenious proposal lately made, of directing a balloon like the tacking of a ship, by means of an inclined plane, is worthy of much consideration. It is obvious that the additional weight of an inclined plane may be avoided, by forming the balloon of some figure not a sphere: thus, for instance, it may be an oblong spheroid, whose major axis is kept inclined at an

an angle of 45° to the horizon, by means of the weight suspended in the car. But a little calculation will show that the lateral motion produced is very small, and not sufficient to counteract any considerable wind; for the whole vertical velocity in the ascent is easily computed, and is not large; and the resolved motion in a lateral direction, being a function of the angle of inclination, is still smaller, and much less than the velocity of any gale of wind.

3. A great number of mechanical contrivances in imitation of wings and oars have been suggested, and even tried, but with a most discouraging degree of success. Upon examining the cause of these failures, it is easy to see that the experiments have been made on principles fundamentally erroneous. In the first place, the power has always been applied to the car, though it is obvious that in such a case the greatest part of the power is lost in giving the car a rotatory motion round the balloon, and that the power, in order to be entirely effective, should be applied in a line passing through *the centre of pressure of the whole system.* In the second place, the mechanism imitated has been that employed by Nature in enabling a bird to fly, though it is obvious that the animal's wings are contrived as much for *support* in the air, as for lateral motion. Our whole attention should be directed to the mechanism of fishes, whose air-bladders assimilate them to an inflated balloon, and in which the system is wholly contrived for the purposes of horizontal motion, progression being produced by the rapid vibrations of the tail, acting like a single oar upon the hinder part of a boat. When we see the rapid progress made by the salmon against the swiftest stream, we should not despair of success; and certainly not on account of the small muscular power of man, if we consider that the steam-engine with the weight of one man commands the power of four. It is indeed a matter of serious inquiry, whether such a machine would not require something more solid to work upon than a metallic poop, or any thing which the balloon could support. It is obvious that much advantage will be gained, if any mechanism acting on the air should move with much greater velocity than the balloon, as the resistance or power increases with the square of the velocity. It will also be a matter of experiment what form of balloon is least resisted; for the received systems on this subject are universally allowed to be erroneous, as the resistance varies as $ar^2 \pm br$ (b being negative in an elastic medium), and as it will probably be found to be a function of the figure of the body resisted.

L. *Queries on Steam-Boats. By Mr.* George Rennie.

To Mr. Tilloch.

Sir, — Whoever reflects on the numerous improvements in the arts and sciences, to which a short but eventful period has given birth, cannot but hail with pleasure the recent introduction of steam-vessels,—an improvement of the highest importance, which discloses a new æra in navigation, and demands every effort for its further improvement. I have therefore sent the following queries in reference to this interesting subject; trusting that such of your readers as may deem an inquiry of this nature interesting, will turn their scientific talents to its further elucidation, which I will also endeavour to do, should not other avocations intervene to prevent me.

I have the honour to be, sir,

Yours, &c.

London, March 12, 1817. George Rennie.

I. Steam-vessels (unlike ships, whose velocity is always irregular and varied at every augmentation or diminution of the wind) have one assigned velocity, according to their respective propelling powers. What is the best form that ought to be given to the head and stern? The extreme length, breadth and depth? Whether flat or round bottom, in order that the requisites of stability, velocity and strength, may attain a minimum?

II. It has been found that the ratio of the resistance being as the squares of the velocities (especially in large surfaces and great velocities) does not maintain; or, in other words, that an engine of a quadruple power will not produce a double effect or velocity:—can any formula be adapted to practice so as to include the circumstances of friction, imperfect fluidity, or adhesion?

III. From the experiments of Dubuat, it is deduced that the whole impulse to be withstood or overcome by the external force is the sum of the plus and minus pressures and of friction; that the non-pressure on the hinder part was prodigiously diminished (1-4th), by making the length of the body triple its breadth; from which he infers that the lengthening of a ship diminishes the resistance;—a paradox, for it supposes a less power to displace a greater quantity of fluid than a small quantity, independent of other circumstances. How far is this consistent with theory and practice?

IV. Admitting perfection to the steam,—to which Mr. Watt to his everlasting credit has been so instrumental;—how far consistent with œconomy, saving of fuel, and attendance on two furnaces,

furnaces, is the adoption of two boilers in place of one, still used in many steam-vessels, and situated athwart the vessel?

V. Two steam-engines have been and still are used in some steam-vessels. What advantage have they over one of double the power?

VI. Steam-engines have been constructed with the beam working above, to the rejection of the beams below. Does not this lessen the stability of the vessel, by removing the centre of gravity higher, and how far is it an improvement?

VII. What is the best situation of the paddle-wheels;—at the extreme breadth of beam, or where? The proper velocity for the floats or paddles? Their area with reference to the section of the immersed part of the vessel? Their properest form? and their depth with regard to their width? The angle best adapted to the velocity of the wheel and vessel; so that no impediment shall arise from the improper displacement of the fluid? and lastly, the number of floats?

VIII. The momentum acquired by a steam-vessel in motion, after a short time, is analogous to that of a fly-wheel;—ought the addition of this fly, adopted in many steam-vessels, to be considered superfluous?

IX. One of the most insuperable objections to steam-vessels, in long voyages across the ocean, must lie in the vast consumption of fuel and bulk occupied by it. How far can this be overcome, without the substitution of some new power? the attainment of which is the grandest desideratum in mechanical science.

X. In the event of steam-vessels being employed in war;—what is the best mode of rendering the paddles shot proof? Would the middle of the vessel render that protection? as without this a vessel disabled under such circumstances, must necessarily be at the mercy of her enemy.

XI. Presuming the above-mentioned difficulty as got over, might not they choose their position? and their distance? By presenting neither masts nor yards, they might cripple their enemy without suffering in turn; their movements would continue all along optional; they might escape from their enemy by steering against the wind. A calm to them is a fair wind.

XII. By presenting no top weight of masts or yards, which are merely temporary, the continual tendency to destroy the hull would be avoided; and all the advantages of trussing on Mr. Seppings's principle could be included, which would at once add stability and strength, rendering her free from the uncertainty of leeway and heave of the sea, and render the reckoning more certain. By being flat-bottomed, and of course drawing little water, the advantages of crossing shallows, and entering harbours

at

at times when other ships cannot; of towing vessels in rivers, and a variety of other useful occupations. Neither, from their extra length and breadth, would they pitch or roll so much. A sail might be occasionally used in lieu of the engine to save fuel.

XIII. It would follow from the last premises, that in action their guns might be pointed with more precision, and the heeling after firing would be less.

XIV. The objections to the employment of steam-boats or barges in canals, is the destruction occasioned to their banks by the violent action of the water during the passage of the steam-boats, and to the want of width in the locks. Would these objections be partly remedied by placing the paddles behind? (for we have all along supposed paddles the best) or could any of the modes hitherto practised for propelling boats answer; so as to be of use in doing away with the number of horses and men usually employed in this service?

XV. Would not the advantages attending a steam-boat, solely for the purpose of towing other boats with passengers and goods, be superior to the present mode, of having the passengers and goods in the same boat as the engine, thereby rendering the vessel weaker by its great length, and unequally strained by the greater or less weight of passengers and goods, which is always liable to vary, and must sink the vessel more or less; thus drowning her wheels or paddles, and consequently obstructing her motion?—independent of those dreadful accidents, frequently liable to arise from bad engines, as lately in America, and one instance in this country.

XVI. Lastly, and politically speaking, are the benefits heretofore stated, as liable to occur by the employment of steam-vessels in war, in navigation and commerce, by defeating our enemies abroad, by facilitating conveyance at home, and rendering cheaper the commodities of foreign countries, to be counterbalanced by consuming with additional extravagance the very vitals of our country (which our coals are) by the great capital consumed in the rapid destruction of the materials of which steam-vessels are composed; and by creating a superabundant population, which have been, still are, and probably will be, the curse of this great and hitherto happy country? G. R.

LI. *On Mr.* Horn's *Theory of Vision.*

To Mr. Tilloch.

Sir,—Your correspondent J. Q. R. not having seen "*The Seat of Vision determined*," has unconsciously attributed some inconsistencies

inconsistencies to Mr. A. Horn for which he is not accountable. Mr. Horn does not maintain that the extremities of the optic nerve are insensible to the impressions of light, while its trunk is endued with a power of transmitting such impressions to the brain. After examination, having satisfied himself that "the retina is nothing else than an expansion of the *scepta*, or membraneous substance that pervades the optic nerve," he divests it of its long usurped sensibility, and limits the extremities of the nerve to the circular portion that perforates the cavity of the eye. The retina he considers a concave mirror, to which the choroides serves as a coating. The inverted pictures of external objects painted on the retina are by its powers as a speculum reflected to the anterior concavity of the eye, where (although there be nothing much resembling a mirror) a second reflection transfers them to nearly "the middle of the vitreous humour, in their natural order and position. *These images make due impressions upon the opposite base of the nerve*, which are transmitted by it to the brain: thus the sensation is produced and vision perfected."

This mode of vision Mr. Horn regards as a real and incontrovertible discovery. Its originality will scarcely be questioned: but as the existence of his images in the vitreous humour depends on two reflections in the cavity of the eye, both destitute alike of proof and probability, others may be inclined to regard it as nothing more than a simple conjecture. Even considered as a conjecture, a single objection* seems sufficient for its destruction. Mr. Horn says that his images in the vitreous humour make *due impressions* on the opposite base of the "(optic) nerve," but *how* these impressions are made he has left entirely unexplained.

This omission it may be feared the sagacity of few of his readers will be able to supply. That the circular base of the optic nerve has the faculty of perceiving images situated at a distance, near the middle of the vitreous humour, or that it can be *duly impressed* by them in such a situation, is a proposition little likely to procure assent without proof. Yet unless some such faculty be shown to exist, it will be difficult to explain *how* these "*due impressions*" are effected; since the pencils of rays constituting these hypothetical images in the vitreous humour, would in their further progress diverge and diffuse themselves

* To both Mr. Pater and J. Q. R. a similar objection occurred. Neither of these gentlemen, however, had seen "*The Seat of Vision*," &c. and neither happened to divine exactly Mr. Horn's mode of *raising images* in the vitreous humour. In consequence of this, the objection is by both urged in a way that Mr. Horn will probably contend does not affect his *discovery*.

over the retina. A small portion of them would therefore fall on the *circular base of the optic nerve*; and the few rays that might fall on it could produce neither erect nor distinct images.

Had we not been assured by Mr. Horn that his "Theory of Vision is *sanctioned by high philosophical authority*," and did I not find it copied, with approbation, into Hutton's Mathematical Dictionary, I should deem some apology necessary for *thus* occupying a page of your valuable Magazine.

I am, sir,

Your very obedient servant,

March 10, 1817. L. S.

P. S. Page 93 of his pamphlet, Mr. Horn tells us, that if a convex lens be fixed in an aperture of a window-shutter in a dark room, and the eye placed in its focus of parallel rays, "no image whatever impresses the organ; a circular spot only is perceived, uniformly tinged with the prevailing colour of the landscape." This none will doubt. But without referring to his pamphlet, it will not easily be credited that he attributes the absence of images in this case to the pencils of rays proceeding from the *objects of the landscape having their foci on the retina!* On this principle (with no little exultation at its discovery) he accounts for the apparent insensibility of the optic nerve, which, according to him, only happens when the pencils of rays proceeding from the pupil have their foci on its base! Surely, if Mr. Horn could for a moment divest his recollection of his imaginary discoveries, he would, like others, perceive that in such a position as he has described the eye with respect to the lens, the pencils of rays proceeding from the objects of the landscape could not possibly have their foci on the retina.

LII. *Letter from Mr.* G. Stephenson *of the Killingworth Colliery: with a few Remarks on his Claim to Priority in the Invention of the Safe-lamp, by the Editor.*

To Mr. Tilloch.

Sir, — I observe you have thought proper to insert in the last number of the Philosophical Magazine, your opinion that my attempts at safety-tubes and apertures were borrowed from what I had heard of Sir Humphry Davy's researches. You cannot have read the statements I considered myself called upon to lay before the public, or you would not thus have questioned my veracity without producing the evidence that induced you to do so. If fire-damp was admitted to the flame of a lamp through a small tube, that it would be consumed by combustion, and that explosion

explosion would not pass, and communicate with the external gas,—was the idea I had embraced, as the principle upon which a safety-lamp might be constructed, and which I stated to several persons long before Sir H. Davy came into this part of the country. The plan of such a lamp was seen by several, and the lamp itself was in the hands of the manufacturer during the time he was here, at which period it is not pretended he had formed any correct idea upon which he intended to act. With any subsequent private communication between him and Mr. Hodgson I was not acquainted, nor can it in the slightest degree affect my claims. That I pursued the principle thus discovered and applied, and constructed a lamp with three tubes, and one with small perforations, without knowing that Sir Humphry Davy had adopted the same idea, and without receiving any hint of his experiments, is what I solemnly assert. To my statement (which may be procured at Mr. Baldwin's) you are bound to give credit, unless by the evidence of facts and dates you are able to disprove it. If you are in possession of any, I call upon you to lay them before the public. If not, as Editor of a Journal professing to be independent, I trust you will acknowledge that you have hastily committed an act of great injustice.

Killingworth, March 15, 1817. G. Stephenson.

I trust I have always evinced my earnest desire to be impartial; and many who have endeavoured to obtrude improper articles into the Philosophical Magazine, by inducements which should never be held out to any journalist, can vouch for my independence.

I had "read the statements" to which Mr. Stephenson alludes. Numbers of people as well as Mr. S. had turned their attention to the devising of a lamp to protect miners against explosions; and some had actually published their ideas, while with Mr. S. all was still only an unpublished idea. To the period of the conception of an idea I attach no importance whatever; and if I may judge of the idea by the form it manifested at its birth, it was an abortion.

Mr. Stephenson's lamp *tried* on the 21st of October 1815, was *not* a safe-lamp. A lamp with one orifice for admitting air, "*with a slide*" over the orifice "*to regulate the quantity to be admitted*," could be nothing else but an exploding lamp; for, to make the lamp burn in atmospheric air, the orifice must be so wide that on going into an explosive atmosphere the combustion could not fail to pass the orifice and explode the mine.

On the 19th of October Sir H. Davy announced that explosion would not pass through small tubes; and

On the 4th of November Mr. Stephenson tried one with three small

small tubes in place of the one opening with a regulator. This he says he "found to burn considerably better" than his first one. "*Better!*" I deny that his first lamp would burn at all with the aperture reduced to that size, by *the regulator,* which would prevent the explosion from passing. He also found this lamp "to be perfectly safe." The first, and I cannot doubt that Mr. Stephenson knows it well, was *not safe.* But even now, on the 4th of November, speaking of this lamp, he says "it did not entirely answer my expectations."

On the 24th of November Mr. S. shows another lamp to Mr. Brandling and Mr. Murray of Henderland, which was tried on the 30th. This instead of small tubes had a "perforated plate covering the air-chamber." But on the 9th of this month Sir H. Davy had communicated his invention to the Royal Society; and that "*wire-gauze stops explosion as well as tubes or canals,* and yet admits *a free draught of air.*" Now what was Mr. Stephenson's perforated plate but gauze of a more clumsy manufacture? Yet have we seen remarks intended to serve Mr. S. tending to insinuate that the gauze was borrowed from his perforated plates!

Let any person of candour look at the evidence, and then say whether I have "hastily committed an act of great injustice" to Mr. S.

But now I have to add another fact, were more wanted. Sir Humphry, after announcing on the 25th of October, to the Chemical Club, that explosions would not pass through small tubes, made no secret of his experiments. Many were invited to see them, and myself among others. I am sorry that I kept no note of the date of my first visit to the laboratory of the Royal Institution where they were made; but from a particular circumstance I am led to believe that it was before the 9th of November, for he made an experiment to show me that an explosion would not pass through wire-gauze, and as far as my memory serves me, he had made the first experiment with gauze that same morning.

Sir Humphry's perfecting improvement followed as a necessary consequence from his discovery that wire-gauze would prevent an explosion from passing downward or upward. If so, how could it pass side-ways were a cylinder of wire-gauze substituted for the cylinder of glass? The glass tube was then thrown away.

I did at first believe that Mr. Stephenson made his discovery, such as it is, independently of Sir Humphry's; but, from an after examination of facts, I found myself compelled to take a different view of the question. The belief or rejection of a fact resting on evidence is not with any person a matter of choice, but of necessity.

A. T.

LIII. *An-*

LIII. *Answer to some of the Geological Queries in our last Number.* *By* N. J. W. *Esq.*

To Mr. Tilloch.

Sir, — It would have given me much pleasure to answer the numerous questions proposed by your correspondent at page 122 in the last number of the Philosophical Journal, respecting several interesting phænomena noticed in the geology of the northern counties: but not having visited every point in question, I must be content with briefly stating my opinion on those facts which have come under my own observation, referring him to Professor Buckland of Oxford, and Mr. Fryer of Keswick, for an elucidation of whatever may appear ænigmatical in the vicinity of Dufton, &c. &c.

1. The same formation which bounds the coast of Northumberland north of the Coquet, abuts upon the eastern side of the Cheviots, and passes their southern limits; and I have every reason to think the coal-mines on Tindal Fell, by which Carlisle is supplied with coals, are situated in this formation; but it is the termination of the Newcastle coal-field that crops out in the vicinity of Staindrop. The tract due west and north-west of the Cheviots consists of graywacke, with the old red sand-stone occasionally reposing upon it.

2. Basalt appears to owe its origin to a different cause from the regularly stratified rocks with which it is associated, never conforming with them for any considerable space. On viewing a range of basaltic eminences stretching through a wide extent of country, or on inspecting a strong vein filled with this substance, one naturally considers the former to be links of a connected chain, and the latter a fissure of vast range: but these appearances are frequently fallacious; for the thickest beds become thin, and soon terminate in the form of a wedge, and the veins as suddenly disappear. For instance: the two large dykes explored at the surface by quarries, and below-ground by mines, at Walbottle and Coleyhill (see the map of Northumberland) were not found a mile or two to the east, either in the workings of Montague main, or East Denton collieries, or intersecting the great level cut from south to north, between Bells, close by the Tyne to Kenton; and the dyke at Walker, so well defined and beautifully delineated by Mr. Hill, has not been noticed in the neighbourhood. The inference is, that all theories respecting the *continuity of basalt* are highly problematical; and that upon treating on this peculiar species of rock, one should be contented to speak of it only when it can be detected *in situ*.

3. Muscle shells, generally filled with clay iron-stone, are found

found in the shales both of the Newcastle coal-field, and in such as do not lie very deep in the encrinal limestone formation generally associated with the thickest seams of coal in that district. The localities of these organic remains are tolerably numerous; but ironstone being an object of little consequence, notice has seldom been taken of its beds or nodules, except when they have been denominated whinstone girdles in the colliery sections. Recurring to our numerous strata of shale, it is impossible to say to which they do or do not belong.

4 and 5. My reason for thinking that the red sand-stone of the Tees is covered by the Whitby alum-rock, has been stated at some length in my tract on the east part of Yorkshire; but I have not as yet been able to ascertain the fact. The paper in question is by no means sufficiently perfect to meet the public eye; but I flatter myself with the prospect of revisiting that part of the kingdom during the course of the ensuing summer.

May I now hope that the gentleman who signs himself A Constant Reader will inform me either by letter, or through the medium of your Magazine,

1st, Whether he considers the Whitby alum shale, and the clunch clay of Lincolnshire, the same stratum; and if the coal-field, covering the former, belongs to it, or is a distinct formation?

2dly, What is the hard blue shell limestone quarried near Therkleby, and how situated with respect to the oelite and coal measures near it? also, what rocks are situated immediately above and below it?

An answer to these questions will greatly oblige, sir,

Your most obedient servant,

Newcastle-upon-Tyne, March 10, 1817. N. J. W.

P. S. As a confirmation of Professor Buckland's position, that the red sand-stone of the vale of Eden is not the *old red sandstone*, rocks of magnesian limestone are associated with it at Low House, between Armathwaite aud Corby.

LIV. *On the supposed Repulsion of Electricity.*

To Mr. Tilloch.

Sir, — As the cause of the divergence of bodies equally electrified does not appear to be generally understood, I have taken the liberty of submitting the following observations, which, if you think proper, you are at liberty to insert in your valuable Magazine.

I am aware that Mr. Singer, in his excellent work on Electricity,

tricity, has given it as his opinion that bodies equally electrified recede from each other, being attracted by the surrounding air. But as my experiments were made before I saw his work, and they do not appear to interfere with his observations, it may not be considered presumptuous in offering them to your notice.

When two balls are electrified positively, the air intervening between them, receiving a portion of the fluid from each of the balls, becomes so highly electrified positively, that the external air is comparatively electrified negatively; consequently the mutual attraction between the air and balls being greatest in the opposite directions *, the balls will recede from each other, being oppositely attracted.

Were the divergence caused by the attraction of the surrounding air, the balls should not recede when electrified in vacuo; whereas if it be the effect of electrical repulsion, the balls should recede still more in vacuo, the resistance being lessened.

To ascertain this, I used an air-pump with a glass plate, and suspended two small pith balls from a wire passing through the centre of a large receiver.

When the receiver was partly exhausted, I electrified the balls, and found the divergence was considerably lessened: but when completely exhausted, there was not the slightest motion perceptible in the balls, although several successive sparks were communicated from a Leyden jar.

I then suspended the balls much nearer one side of the receiver than the other: and when electrified in vacuo, the result was, that they were *both equally attracted* towards the nearest side of the receiver.

It therefore appears to me that we are justified in concluding that the recession of bodies equally electrified is caused by the attraction of the surrounding air: and certainly this hypothesis is the most simple, and most congenial to the maxim—that we should never bring two principles into action when only one is necessary. I am, sir, yours, &c.

W.

LV. *On the "Fixed Oil of Wine;" and on the Light emitted by metallic Wires of low Temperature in certain volatile Media. By Mr.* J. Farley.

To Mr. Tilloch.

Sir, — It was my intention (at no distant period) to have troubled you with an account of a few experiments for insertion in your

* It is well known, that a body positively electrified will have a greater attraction for another body, in proportion as the other body is negatively electrified.

Magazine; but conceiving it highly imprudent to interfere with the labours of others till they had been *regularly* published, I was induced to refrain a while, anxiously waiting the arrival of that period. Nor should I address you at present, had I not observed (on perusing your last number, page 121,) that I have been anticipated by Mr. Murray in a manner which I did not expect. Soon after Sir H. Davy's brilliant discovery (of the curious phænomena presented by platinum and palladium wires in inflammable media) was made known, I was induced to try the effect of the first metal in the vapour of oil of wine, where it continued "to glow very vividly until the ethereal matter" was "all expended;" when I discovered the "peculiar fixed oil" remaining in the vessel. I united it with the alkalies, and satisfied myself that it was a "peculiar fixed oil" (which indeed I then termed it), by its unctuous feel, and leaving a distinct semitransparent stain on paper after having been heated. I ascertained that it was "highly inflammable," and of less specific gravity than water.

Mr. M. happening to call on me a few days after, I informed him of these experiments and their results; though I by no means wish to insinuate that he has not repeated them: nor should I have thought it worth while to notice this, had I not intimated to him (at the time) my intention of communicating them to you; and furthermore, received his entire acquiescence in its propriety. Should you be enabled to find room to insert this in your next number, you will much oblige, sir,

Your humble servant,

London, March 18, 1817. J. Farley.

P. S. From my experiments I should be inclined to consider oil of wine to be sulphuric ether holding in solution the "peculiar fixed oil," and which is most probably formed by an alteration in the play of affinities exerted during the latter part of the process for obtaining sulphuric ether. The "peculiar fixed oil" (which is of a light yellow colour) may be obtained by merely evaporating oil of wine with a gentle heat.

LVI. *Remarks on Mr.* Frend's *"Evening Amusements." By the Rev.* T. Drummond.

To Mr. Tilloch.

Sir, — "No man is wise at all hours" is a quaint remark, and I must rely on the appropriateness of its application to apologize for the introduction of it at the commencement of my letter. The valuable annual publication by W. Frend, Esq. has contributed, in no slight degree, to promote a familiar acquaintance

ance with the positions of the starry host, and the appellations which are usually given to the most conspicuous amongst them.

Evening Amusements for 1817 has this day been received, completing my set of a work which I estimate not meanly.

The patriot, the philosopher, the admirer of civil and religious liberty, may by some of his readers be suspected to have been *planet-struck*, or under the dire potency of *lunar influence*; but with different impressions I have perused animadversions which do not manifest that spirit of liberality which I have long considered one of his characteristics. If a few animadversions on the subject are admissible in your pages, I beg leave to offer them.

There are some remarks on which I shall say nothing, not being disposed at this time to argue in defence of the *dark-eyed, black-haired, wandering Sibyls*, nor of those who practise what is commonly denominated *astrology*; neither have I any charge to bring against the *spots* on the sun on account of the late rainy season. But that rain is produced by peculiar changes in the atmosphere, and there must be some preceding operations to occasion those changes, I presume is indisputable. The question then is—why not the motions of the planetary bodies?

Mr. Frend is not called upon to subscribe to the doctrine of planetary influence: but as dogmatism in science as well as religion, whether on the orthodox or heterodox side of the question, is not amiable;—I wish to enter my protest against a sentiment which he publishes without any argument to support it. Whatever may be the views of the writer of Moore's Almanack, I am persuaded that, in avowing my opinion of planetary influence on the atmosphere, I do not act under a *delusion*, and feel confident that I harbour no wish to *delude others*.

In *opinions*, as in the *waters*, there may be *flows* and *ebbs*; and some persons do not hesitate to ascribe them alike to the influence of the planets. Whatever unknown cause may produce the former, I cannot see why the correctness of the now generally received opinion respecting the latter should be denied.

I remember that in 1811, on reading *Evening Amusements* for February, I inferred that Mr. Frend was a believer at least in atmospherical astrology; and although the publication for the present year is replete with sentiments in OPPOSITION; perhaps, like the *planets* themselves, they may appear RETROGRADE for a season, and then become DIRECT.

We read, p. 21: "Freedom of mind is a blessing which cannot be appreciated by those who have it not, nor too highly valued by those who are in possession of it." Again: "I write

for those who will allow me the same freedom I wish them to enjoy." This is rational, this is friendly.

I presume the work is in the hands of your readers, and shall forbear making many extracts. There is one in 1811 I must quote, and two short paragraphs in 1817, which cannot be passed unnoticed. The writer of Moore's Almanack is harshly treated, the speculators on the changes of the moon are severely lampooned; and although it is consistent with freedom of thought to admire the wonderful scene of the constellations, and the grand progress of the moon among her starry contemporaries, thus far we are allowed to be free, and no further.

Mr. F. says there are numerous diaries kept, relative to the weather "not only in our own island, but upon the continent. I have not thought it worth while to examine any of them, but I know a very respectable clergyman who kept a diary of this kind for thirty-five years, and he upon examination could not find the least connexion between the moon and the weather." This, sir, is an instance of implicit confidence I never expected to hear from a philosopher:—the mode of observation is unknown, it might be totally erroneous; but as Mr. F. did not think it worth while to examine it, I must withhold my assent to the inference, because I cannot understand that which is veiled in all the darkness of unintelligibility.

Mr. F's invective against the writer of Moore's Almanack respecting the weather must, I apprehend, be founded on his presumption to exercise his *freedom of thought*, and in his expressing himself occasionally with less positiveness than some might expect from a firm believer in the science of atmospherical astrology.

Evening Amusements, Feb. 1811. "On looking at the space marked out for water, the turbulence of the ocean will arise in his mind. The mighty agitation of waters, by the *influence of the moon*, will excite his attention."**** In fact, A HIGH TIDE *may be expected* this month. **** Persons by the sea-side, or on the banks of tide rivers, will be upon their guard on this account for the 24th. **** It may be only a very strong and high tide," &c.

I confess I could not divine Mr. Frend's ground for apprehension; and as his prediction failed, I considered it as one of those errors to which, without further improvement in the astro-science, we are all liable.

In 1817, we read: "As the diaries convince us that all these surmises respecting the moon's influence on the weather are fallacious, so I am inclined to believe the same will be discovered to be the case on its pretended influence on the tides. **** And I apprehend that the whole doctrine of the influence of the moon on

on the tides will share the same fate with that of its pretended influence on the weather."

Having advanced enough to prove that Mr. Frend was in *propria persona* a conjecturer in 1811, I leave others, whilst reflecting on his present opinion, that *gravity, attraction, the moon's influence on the tides*, &c. are mere chimera, to assign him his proper station, either in or out of the ranks of modern soothsayers. As I continue to acknowledge the benefits we receive by the agency of the sun, notwithstanding the spots that are discernible on its disc; so I shall not cease to consider the *Evenings Amusement* to be a truly estimable work, although I may not be persuaded by any thing contained therein to revoke my opinion, that not the moon only but the constellations, the moon and the other planets, are the agents in producing the changes in our atmosphere.

Yours respectfully,

Gray Friars Priory, Norwich, Jan. 13, 1817. T. Drummond.

LVII. *On the Exhibition and Harmonic Effects, of The Rev. Mr.* Liston's Euharmonic Organ: *By* A Correspondent.

To Mr. Tilloch.

Sir, — The exhibition announced in p. 464 of your last volume, has at length begun to take place, and doubts can no longer be entertained, whether *compound Stops* on Organs, capable of being *perfectly* tuned, would be equally *gratifying to the Ear*, with chords on simple stops, involving none or fewer discords in their composition; and the results have been most satisfactory and gratifying to several select parties of Professors and Amateurs. *Mr. Purkis* displayed much taste and execution in exhibiting the Instrument: among the many full pieces which he played, was the Hallelujah Chorus in 5 sharps; the very novel and fine effect of which, struck every one. Several of the most unusual and extraneous modulations and enharmonic changes of modern Composers were exhibited, with new and very gratifying effects, such, as perhaps the Composers themselves were in part unaware of. In my latest visit at Messrs. Flight and Robson's a printed paper was put into my hands, which I wish to see preserved in your pages: it seems to proceed on, and with good effect to follow up, what is stated in vol. xxxix. p. 420, and other parts of your very valuable work.

I hope some of the able Professors and Composers who are seen

seen among the hearers of Mr. Liston's Organ, will favour the Readers of your work, with their free and ingenuous opinions and remarks, on its merits and its defects, if any such they discover, since the Philosophical Magazine has long stood distinguished amongst its cotemporaries, for its correct and luminous expositions, of the mathematical principles of Music, and none will be thereby more gratified, than, sir,

Your very humble servant,

PHILO-MUSICUS.

The paper above mentioned is as follows; viz.

"*To the* MUSICAL PROFESSORS *and Amateurs, who may attend to try or hear* MR. LISTON'S *Patent large, Euharmonic Organ, with compound Stops, exhibiting at Messrs.* FLIGHT *and* ROBSON'S *Room, in St. Martin's Lane*, 1817.

"Mr. FAEEY *Sen.* with the view of aiding the perfect conception and knowledge of the grand improvement made by his friend *Mr. Liston*, begs thus to offer a few *numerical facts*, and particulars, entirely divested of the *mathematical form* in which they have been derived by him* and demonstrated, as to *the true values of the several Intervals* on Mr. Liston's curious and important Instrument.

"There are in the Scale of 39 Notes on this Instrument, two exceedingly small, and yet highly important Intervals (called *Schismas*), between C♭ and B′, and between C‵ and B′*, which prove to be *the* UNITS *of the Scale*; in terms of which, every possible Interval on this Intrument, or even on one of a far more extended Scale, can be *arithmetically expressed* and compared, with the utmost ease and accuracy, by every Musician who is ready at *addition and subtraction;* qualifications, in which it would be a libel on the Readers of this, to suppose them wanting.

"The major *Comma* is 11, the major *Semitone* is 57, the minor *Tone* is 93, the major *Tone* is 104, the major *Third* is 197, the major *Fifth* is 358, and the *Octave* is 612, of these Schisma Units, respectively.

"The tuning of this Organ, has been wholly conducted by help of the three last mentioned concords, the *Third, Fifth*, and *Octave*, each tuned *quite perfect* (without even the slowest *Beats*, which it was possible to detect), and according to the process which is described in the first 7 and in the 44th and 45th pages of Mr. Liston's "Essay on perfect Intonation" (which is on Sale at the Music Shops), or in p. 421 of Phil. Mag. vol. xxxix. Now following this process arithmetically, double 358 for the two Fifths CG, Gd, tuned upwards from C, and deduct 612, for the

* "In a Series of Letters in the *Philosophical Magazine*, commencing in its 28th Volume in 1807, and since often continued: in the articles of the *Edinburgh Encyclopædia;* in the *Monthly* and *Gentleman's Magazines*, &c.

the descending Octave to D, and it will thence appear, that 104 Schismas compose the Interval CD; then, since E the Third above C, is 197, a fifth tuned upon or added to this, gives 555 for B; and so on, through the whole 39 Notes; which Notes arranged, are as follows, viz. 0(1), 11, 36, 47(I), 57(2), 93, 104(II), 140, 150, 161(3), 197(III), 208, 233, 244, 254(4), 265, 290, 301(IV), 311(5), 322, 347, 358(V), 369, 394, 405, 415(6), 451(VI), 462, 498, 508(7), 519, 544, 555(VII), 565(8), 566, 576, 591, 601, 602, and 612(VIII). Where the *numeral* designations of the Intervals are shown in parentheses; viz. 1, I, 2, II, 3, &c.; answering to C, C*, Db, D, Eb, &c. and the Schismas fall between 565 and 566 and 601 and 602.

"By adding 612 to each of the above 39 numbers, and setting them down as a second Octave above this, and then going successively through the arithmetical *subtraction* of every number above given, from each of the 39 numbers next greater than it, every possible Interval on this Organ, will thus be shown or measured, without the least fractional differences or errors. It is hoped, therefore, that the chief *difficulty* heretofore opposed, to the *readily understanding* of the enlarged or *true Scale of Music*, is now removed.

"It may be proper to add, that the Schisma Unit, above described, taken from a true Fifth, produces the proper *Equal Temperament Fifth*, being 357; of which *tempered Fifth*, this Organ furnishes five Examples; viz. E* c`, E′ cb, B′* g`, B′ gb, and G′* e`b, either of which will exhibit to the ear, the true effect intended, as to deviation from perfection; but none of them fall in the proper places of the scale, for the Isotonic System of Temperament, whose Notes must all be multiples of 357, abating Octaves; because 357, multiplied by 12, is exactly equal to 612 multiplied by 7; and multiples also of 51, the Half-note of this System; because 51 multiplied by 12, makes 612. So with regard to any other Tempered Systems, their Intervals may be expressed in these numbers and decimals; thus, in the Mean-Tone System, where the Fifths are flattened $\frac{1}{4}$ of a Comma, or 2·75, the Fifth is 355·25, four of which CG, Gd, da, a*e*, make 1421, from which take 2 Octaves or 1224, and 197 remains, for the *perfect* major Third CE, see Phil. Mag. xxxvi. pp. 39 and 374.

Howland-street, March 25, 1817.

LVIII. *Notice on the Origin and Practice of the new Invention of Lithography.*

Aloys Sennefelder, a singer at the theatre of Munich, was the first who observed the property which calcareous stones pos-

sess of retaining lines drawn with an oily ink, and of transferring them in all their strength and purity to paper, applied with a strong pressure upon their superficies. In 1800 he obtained from the King of Bavaria a patent, securing to him the exclusive benefit of his discovery for the space of three years; and in concert with a Baron d'Aretin he formed at Munich a lithographic establishment, which still exists.

D'Aretin afterwards joined with a M. Manlich in forming a new establishment, more calculated to accelerate the progress of the art; and among other works which it has produced, it has just occasion to boast of an excellent collection of copies from the great masters which adorns the cabinet of the King of Bavaria.

Count Lasteyrie, a French nobleman, struck with the advantages of this invention, made several journeys to Munich, in order to instruct himself in it; and afterwards attempted, but without success, to establish the art in Paris. The Count is said to have composed a treatise containing the whole details of the process; and it will be unfortunate, since that process is still involved in some mystery, should this treatise not be made public.

The difficulties which opposed the attempt of Count Lasteyrie in the capital did not, however, prevent the establishment of the art in a remoter part of France. At Mulhausen on the confines, a M. Engelmann afterwards introduced and cultivated it with great success; and very recently he has transmitted to the Society of Encouragement at Paris a number of lithographic specimens, which have excited a great deal of attention in the admirers of the fine arts.

Some of these specimens are in crayon; others have been executed with a pen or pencil; and there are also some done in the manner of wood-cuts. The whole of them are remarkable for great delicacy of touch, and the mellow impression which they make on the eye; though it may be observed that in those which are in the manner of wood-cuts, the light and shade are less harmoniously blended than in the others executed with a crayon or pencil.

The whole details of the mode of execution are not revealed; but enough is known to enable a general idea to be formed of the process.

Although similar to engraving, lithography is yet something very different. It resembles engraving in no other respect than in as far as it is an art of multiplying indefinitely the same design. It has the advantage over engraving, of producing not *copies* of designs, but the designs themselves—the original works of the designer repeated as often as impressions of them can be taken. Lithography then may be regarded as the

art

art of multiplying originals; engraving, as that of multiplying copies.

The facts on which the lithographic art is founded are the result of affinities simple enough, but the applications of which have not been observed.

1st. It is a fact that a line drawn with a crayon, or an oily ink, upon stone of a calcareous nature, adheres to it so strongly, that it can only be taken off by mechanical means.

2dly. All the parts of the stone not covered with the oily matter will receive, retain, and absorb water.

3dly. Suppose, then, that any combination of lines has been drawn upon the stone, and that it has afterwards been thus treated with water,—if a coating of oily and colouring matter be next applied to the stone (as in letter-press printing), it will only attach itself to the lines formed by the oily ink, and will be rejected by the moistened parts.

The lithographic process, in short, depends on this: that the stone, moistened with water, rejects the ink; and when oiled, that it rejects the water and retains the ink: so that in applying and pressing a leaf of paper upon the stone, the oily, resinous, and coloured lines will alone be transferred to it, presenting the counter-impression of what is represented on the stone.

The design, instead of being traced in reverse upon the stone, may be drawn with prepared ink, on paper, and thence transferred to the stone; by which means the stone will then return the exact image of the original.

All stones will do for the purpose, which are susceptible of being penetrated by any oily substance, and of imbibing water with facility; provided they are compact, of a clear and uniform colour, and capable of receiving a fine polish. The whole of these qualities are very happily combined in certain calcareous stones, which are found in the quarries of Solen Hofen, near Pappenheim in Bavaria, and may be met with, it is believed, in several other places. They consist of almost pure carbonate of lime.

When the stone is dressed and polished, the artist may, without any other preparation, sketch his design upon it, either with crayon, pen, or pencil. The grain of the stone being more fine and equal than that of the finest and best pressed vellum, he will find himself enabled to draw his lines with so much the greater equality, firmness, and delicacy.

The mode of taking off the impressions from the stone is that part of the process which, it is to be regretted, is still kept in some degree of mystery, or rather, it should be said, which has not been communicated; for it is difficult to imagine that there can be much mystery to any scientific artist in an operation which is so well understood, and has already been carried to such

such a degree of perfection. The press which M. Engelmann uses, is described as "different from all other presses;" but we are not aware of any thing which can be wanted in point of force and equality of compression, that may not be obtained by means of the ordinary copperplate or rolling presses; by the latter especially, which upon the whole we should think will be found best adapted to the lithographic process.

In order to ascertain the full value of this invention, it remains still to be known whether any given number of impressions *equally* beautiful can be taken, and what that number is. In the first essays of the art, it is certain that a very great inequality in the impressions was experienced; and although it is said that M. Engelmann has devised means of apportioning with the most scrupulous exactness the quantity of the different ingredients, and of retouching his designs from time to time, no results are stated to enable us to form a satisfactory conclusion on the points we have stated.

It would be also desirable to know at what price the lithographist can furnish his impressions, since this must have no small influence on the success of any competition between them and engravings.

LIX. *Notices respecting New Books.*

NEARLY ready for publication, A Translation of Thenard's new Treatise on the general Principles of Chemical Analysis; with Plates and valuable Additions from his Elements of Chemistry, &c. forming one octavo volume.

A Second Edition of The Amusements in Retirement; or, The Influence of Science and the Arts on the Manners and Happiness of Private Life, will be published in a few days.

Sir William Adams is about to publish A Practical Inquiry into the Causes of the frequent Failure of the Operations of extracting and depressing the Cataract, and the Description of a new and improved Series of Operations, by the Practice of which most of these Causes of Failure may be avoided.

A new Edition of Dr. Thomson's System of Chemistry is in the press, and will speedily be published. The work will be entirely remodelled, and will be comprised in four octavo volumes.

Dr. Leach, of the British Museum, has recently printed a very complete Catalogue of Birds and Quadrupeds which are natives of

of Great Britain. It is perhaps the most correct catalogue which in our present imperfect knowledge of British ornithology has been as yet compiled. But he has made (we do not know on what authority) very considerable alterations in the old arrangement, and composed a variety of new English generic names.

A catalogue has likewise just issued from the press, entitled *Catalogus Avium in Insulis Britannicis habitantium, curâ et studio Edwardi Forsteri jun.* This is merely a catalogue of birds discovered wild in Great Britain. Mr. Forster differs also in this catalogue in his arrangement from the arrangement and names of Linneus. He enumerates 293 species.

Another new work On the pernicious Influence of Wine and fermented and spirituous Liquors in general, is about to be published; containing Preliminary observations on the principles of health, and on the extensive application of the doctrine maintained by Mr. Abernethy (in part i. of his Surgical Observations respecting the sympathetic influence of the digestive organs) to the different genera and species of diseases; and on the periodical influence of atmospheric causes on our health.

Mr. J. Robertson, of Surry House Academy, Kennington Cross, will in a few days publish A Practical Example Book on the Use of Maps; containing problems and exercises to be worked and filled up by students in geography. Designed as an auxiliary to that study for the use of schools and private students.

An interesting pamphlet "On the Accidents which occur in the Mines of Cornwall, in consequence of the premature Explosion of Gunpowder in blasting Rocks, and on the Methods to be adopted for preventing it, by John Ayrton Paris, M.D.F.L.S." has just made its appearance.

The same gentleman is now preparing for publication A Descriptive Catalogue of the Geological Specimens deposited in the Museum of the Royal Geological Society of Cornwall; interspersed with observations tending to show the œconomical applications of geology to the agricultural, mining, and commercial interests of the county of Cornwall. This work will form one volume octavo.

To Mr. Tilloch.

Sir,—I beg the favour of correcting, through the medium of the Philosophical Magazine, a typographical error in my Essay on Galvanism recently published, which entirely destroys the sense and force of the experiment, and which heretofore escaped my attention. The passage, beginning page 277, line 26, runs thus:

thus: "the other wire was inserted into a disc of copper, in a similar manner." It should be read, "the other wire was inserted into a disc of cork, in a similar manner."

I hope that such readers of your Magazine as have copies, will take the trouble to correct the error.

I am, sir, with respect, &c.

Dublin, March 13, 1817. M. DONOVAN.

LX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 27. AT the instance of the President, Sir E. Home furnished some descriptive observations on the fossil bones found by Mr. Whitby, the superintendant of the works, in the quarry whence the stone for the Plymouth Break-water is extracted. These bones were found about 70 feet below the surface of the ground, and four above high-water mark, in a cavern which is nearly opposite to, and at a little distance from, the works now carrying on at Plymouth. The bones are more perfect, and freer from extraneous matter, than any other fossil bones hitherto found. The cavern has no incrustations on its sides, no external communication, and no appearance of infiltration; its bottom, in which the bones were deposited, is filled with clay, three feet of which covered them. The bones found by Mr. Whitby, and sent by him to Sir Joseph Banks, belonged to three different animals of the rhinoceros species, and are larger than those in Mr. Brooks's Museum. According to Mr. Brande's analysis, they contain little but the usual contents of bones, and have very little earthy or extraneous matter. It appears that there are two kinds of stone used in constructing the Break-water, and that the one being much harder than the other, a different price is paid for raising it; but the part in which the bones were found is of the harder kind, and for which the greatest price is paid for quarrying.

March 6. The Rev. Mr. Hyde Wollaston gave the Society a description of an instrument which he has lately invented for measuring altitudes, and which he calls a thermometrical barometer. Every one who has attempted to use the common mountain barometer has experienced the difficulty, and almost impossibility, of preserving it without fracture. To remedy this, Mr. Wollaston, observing the striking difference in the temperature of water according to the atmospheric pressure, instituted a series of experiments, in order to construct a thermometer which should answer all the purposes of measuring heights with extreme

treme accuracy, and at the same time be more portable, less liable to break, and in every respect more useful than the barometer. He began by taking small thermometric tubes, giving them large bulbs, and extending them to three inches for every degree of Fahrenheit's scale. These he found very sensible; but for general purposes he considers that one inch to every degree of Fahrenheit (each of which he divided into a thousand degrees) is the most convenient; that the capillary tubes should not be so small as to present any great resistance to the expansion of the mercury; and that a tube of common diameter, having a bulb inserted in a metal box about four inches long and 1·4 wide, will indicate every foot of elevation. To this box water is added, and a small lamp placed under it, so that the water may boil; and as the operator ascends, the ascent of the mercury at the boiling point indicates the difference of height between any one place and the common surface of the earth, or the level of the sea. Mr. Wollaston made an experiment with his instrument by boiling water on the counter of a bookseller's shop in Pater-noster-Row, and again boiling it in the dome of St. Paul's, and found that this thermometer exactly corresponded with the geometric measurement of that building.

March 13, 20, and 27 were occupied in reading part of a long and elaborate paper on the natural history of cinnamon, by Mr. Marshall. The author began with a botanical description of the *Laurus Cinnamomum*, in which he corrected the errors of all preceding writers, and particularly those of Thunberg, who has been generally followed by subsequent botanists. He described the soil, modes of gathering it, packing it with pepper on board of ships, specific differences or varieties of cinnamon, its names in various languages ancient and modern, &c. It appears that oil of cinnamon is made by distilling the fresh bark with salt water; and that 80 lbs. of bark yield only $2\frac{1}{2}$ ounces of volatile or supernatant oil, and $5\frac{1}{4}$ ounces of a darker coloured and heavier oil: that the former floats on the surface of the water, and the latter sinks to the bottom; so that both are easily collected from the water, and from each other. Somewhat less oil is obtained from dry bark.

The Society then adjourned over, in consequence of the holidays, two Thurdays.

BATH LITERARY AND PHILOSOPHICAL SOCIETY.

Monday, Feb. 17. Mr. Cranch communicated to the Society the substance of some papers transmitted to him from Dorchester, near Boston in New England, relative to a *mummy* discovered in an immense subterranean cavern in the State of Kentucky.

The

The mummy is that of a stout woman nearly six feet in height, though the whole *materiel* is so intensely *dry* as to weigh but twenty pounds.

It was found in the cavern at the distance of *three miles* from its entrance. The figure appeared seated in a sort of rude sarcophagus composed of five limestone slabs; the fifth stone serving as a cover or entablature to the rest, exactly similar to the ancient *cromlechs* still extant in various places of the British islands. The knees had been brought close up to the body;—the hands were clasped upon the breast;—the head, covered with something like a coronet, was erect;—and the whole figure was muffled up and covered with a number of garments made of wild hemp and willow bark. Several bags containing beads, trinkets, and various handicraft implements were lying by the body, with a sort of work-basket, a curious musical instrument, and a fan made of feathers *à la Vandyke*.

The entrance of the cavern is forty feet high by thirty feet wide, and for some years past saltpetre has been made and oxen worked as far as two miles within it. A Mr. Ward has recently explored this wonderful cavern to the extent of *ten* miles. He says that after having proceeded some miles they ascended a vertical chimney-like passage, and climbing up from one stone to another about forty feet, they entered *at midnight* a chamber 1800 feet in circumference, and 150 feet high in the centre! From this chamber they proceeded about a mile further, and how much further they might have gone they knew not. In another chamber which they traversed, they were presented with a scene to which there is at present, perhaps, no parallel in natural history—a single arch of solid rock 100 feet high projecting over an area of not less than *eight acres!* From the observations which they made, they fully satisfied themselves of this further astonishing fact,—that Green River, a mighty stream navigable for several hundreds of miles, must necessarily have passed over their heads in three different branches of the cavern.

A great many discoveries, it is added in the communication to Mr. Cranch, have been made in Kentucky, which indicate the existence at some very remote period of a state of society, arts, and social habits far more advanced than any of the aboriginal tribes hitherto known have exhibited.

A paper by Dr. Wilkinson On the Rise of Fluids in capillary Tubes was afterwards read. Its object was to show that the experiments on this subject do not accord with the theoretic calculations of Professor Atwood of Cambridge, and other philosophers;—that the results do not correspond with any fixed rule, but are entirely dependent on certain conditions of the tube; and

and that no dependence can be placed on any deductions from mathematical investigations as to what has been termed *capillary attraction.*

Monday, March 10. Dr. Wilkinson stated, that he had received a letter from Mr. Bakewell, author of The Elements of Geology of England, intimating that he had discovered in the neighbourhood of Berkeley, Gloucestershire, basalt with coral and shells imbedded in it; a circumstance difficult to reconcile with the theory of basalt being of igneous origin. The learned Doctor also exhibited a specimen of the fruit of the locust-tree, the *hymenæa* of the West Indies. It is a large pod containing seeds surrounded by a saccharine pulp. Dr. W. remarked, that it had been supposed by some, that this constituted the food of St. John in the Wilderness. In Arabia, Numidia, Abyssinia, and even among the Hottentots, the locust insect is a favourite article of food, and used both in its recent and preserved state. The locust alluded to in the Scriptures, is supposed to mean literally the insect, and the wild honey a species of manna.

A communication, from a member, relative to the appearance of the *aurora borealis,* as seen in Derby and its neighbourhood on Saturday the 8th of February last, was next read. It was observed by Mr. Horner, that the reappearance of these lights militates against the theory advanced by Dalton and Darwin, of the coincidence of the vertex of these irradiations with the line of no variation of the needle. The variation of the needle, as observed at different periods at Greenwich, is as follows:

In 1580,	11°	15′	eastward	In 1745,	18°	6′	westward
1622,	6	0	ditto	1751,	19	0	ditto
1634,	4	5	ditto	1772,	23	30	ditto
1657,	no variation			1786,	26	21	ditto
				1790,	27	15	ditto

Between 1657 and 1790 the annual average was nearly 12° 18″. After having been many years stationary, the needle is now returning to the north.

The discussion of Dr. Wilkinson's Theory on the Rise of Fluids in capillary Tubes next occupied the attention of the Society. The Doctor remarked, that from experiments it appeared that the rise of fluids is not exactly as the reverse of the diameters, but nearly so; the columnar pressure of fluids being as the area of the base multiplied by the perpendicular height. Hence with equal heights the pressure will be as the squares of their diameters; so that in a tube of one-tenth of an inch, and another of one-twentieth, to render the quantum of water in each tube the same as asserted by Professor Atwood, the fluid ought to rise four times higher in the smaller tube than in the larger, which

noway

noway accords with the experiments made. Whatever quantum of fluid any tube will suspend, will also be the measure of the mechanical resistance of the fluid with respect to the tube. If the columnar pressure and the resistance were the same, there would be no difference in the levels. As the ratio of the columnar pressure exceeds the resistance in the same degree as the squares of the diameters exceed the proportion of the diameters, so the fluid will rise to a height above the level of the fluid exterior to the tube in the same proportion. Supposing a tube immersed one inch in a vessel of water, and the resistance equal to half an inch—in this state the fluid will rise half an inch within the tube; and it might then be supposed that a balance would take place, and that a depression instead of an elevation of the fluid would be the consequence. In this position the columnar pressure of the fluid becomes as nothing, for it is supported by the mechanical resistance of the sides of the tube: hence the pressure of the external fluid determines more towards the tube; and the operation may be considered as continuing until the fluid within the tube is of the same height as the fluid on the outside, *plus* that portion which is in the measure of the resistance of the fluid against the sides of the tube.

The resistance of fluids against the sides of tubes Dr. W. referred to the same mechanical cause which occasions two leaden hemispheres with polished surfaces to cohere most powerfully when brought into contact. This cohesion, the Doctor observed, does not take place, unless a rotatory motion is induced on the hemispheres when in contact; nor can they be easily separated unless by a similar motion. When the surfaces are examined, particles of lead appear raised up from the surface of each hemisphere, and closely locked together; the resistance to separation is in proportion to the number of particles thus entangled; but unless the metal is soft, no such effect takes place. Experiments were made with hemispheres of lead and tin, and tin and brass. It has been stated that polished brass hemispheres will strongly adhere, if their surfaces be smeared with oil. Dr. W. maintained that the interposition of any substance between metallic plates must tend to keep the metallic surfaces at a greater distance, contrary to the admitted laws of attraction being in the inverse duplicate ratio of the distances; the cohesion in this instance he conceived to be merely referable to the tenacity of the oil.

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

At a meeting of this Society on the 24th of February last, at Penzance, a paper was communicated by J. H. Vivian, Esq. Vice-

Vice-President, "On the Methods of making the different Preparations of Arsenic in Saxony, and on the Process of preparing Smalt or Cobalt in Bohemia;" accompanied with illustrative specimens and drawings. The author observed, that during his travels upon the continent he had had the good fortune to be admitted into the different mining works; and that as he examined them in detail, he considered that by giving publicity to such information he might be of service to the county of Cornwall, especially as he had heard that an establishment in the county had to contend with very considerable difficulties in making the different preparations of arsenic; and it was notorious that cobalt was imported from Saxony at the very time that ores of that metal were found in very considerable quantities in Cornwall. A large collection of original and beautiful drawings illustrative of the different machines used in the Saxon mines, together with several models and a complete dress of a Saxon miner, were also presented by the author.

Dr. Paris presented to the Society a *geological pillar* intended to exhibit in one view all the different rock-formations, and the relative order in which they occur. The pillar is fourteen feet in height, and composed of spiral shelves, which carry specimens of the different rocks and wind from the base to the capital round a central shaft; the whole pillar is made to revolve upon its base, so that each specimen may be successively brought into view. Its appearance is altogether very novel and striking. Dealers in minerals will probably derive an useful hint from it, and adopt a similar method for exposing their specimens to inspection.

The Rev. Wm. Gregor communicated some specimens of a mineral of rare occurrence in Cornwall, but which has been lately found in Pengelly Mine in the parish of St. Ewe; it is a compound of arsenic and nickel, and has the name of *kupfernickel*.

Dr. Paris presented a most interesting account of the accidents which occur from casual explosion in the mines of Cornwall, and on the methods of preventing it.

A Committee was appointed to devise means for establishing, with an appropriate endowment, a Professorship of Mineralogy and Geology.

ROYAL ACADEMY OF SCIENCES OF PARIS, *17th March* 1817.

The prize established by M. Lalande, for the most interesting observation or most useful memoir in astronomy, was for this year decreed to M. Bessel, director of the Royal Observatory of Konigsberg.

The late M. Ravrio having often had occasion to observe how

much the art of gilding with mercury is injurious to the health, left a legacy of 3000 francs in favour of the person who should discover a process by means of which mercury might be employed without any danger in gilding. Government having approved of this legacy, the Academy published last year a program descriptive of the art, the inconveniences of which are sought to be removed; and pointing out those particular operations in which the danger is greatest: but no satisfactory memoir on the subject having been presented, the Academy resolved to propose the subject anew for the year 1818. It is required that the competitors shall put in practice in some workshop in Paris the processes which they propose; and that these processes shall be such, that besides effecting the principal object in view, they may combine some means of re-collecting the evaporated mercury. The period of competition is limited to the 1st of January 1818, and the result will be published the first Monday of March 1818.

Two other prizes which the Academy had announced last year remaining also unmerited by any of the memoirs which they have called forth, the Academy resolved to propose them in like manner again for the year 1818.

Both prizes are gold medals of the value of 3000 francs each.

The subject of the first prize is, "To determine, 1. The rise of the thermometer in mercury comparatively with its rise in air from 20° below zero to 200° centig. 2. The law of cooling in a vacuum. 3. The law of cooling in air, hydrogen gas, and carbonic acid gas, to different degrees of temperature, and according to different states of rarefaction."

The subject of the second prize is, "To determine the chemical changes which fruits undergo during and after their maturation."

The Academy also resolved to propose the following subject for another prize in physics, to be adjudged in the public sitting of March 1819: "To determine by accurate experiments the defraction of luminous rays, direct and reflected, when they pass separately or simultaneously near the extremities of one or many bodies of an extent either limited or indefinite, having regard to the intervals of these bodies; as also to the distance of the luminous body from which these rays emanate. 2. To deduce from these experiments by mathematical induction the motions of rays in their passage near such bodies."—The prize to be a gold medal of the value of 3000 francs; and the period of competition limited to the 1st of August 1818.

SCIENTIFIC

SCIENTIFIC ESTABLISHMENT, WURTEMBURG.

Wurtemburg, Feb. 20.

His majesty the King, convinced of the advantage which a union of respectable men of letters affords the state, has resolved, by a rescript of February 17, to give to the scientific establishment in the capital such a form as may make it possible for such men to apply with success to the various objects of their researches; and has ordered the union of The cabinet of medals, coins, works of art, minerals, and natural history, with The royal public library, reserving the rights of the royal house to these collections. The King has appointed Dr. Kielmayer, hitherto professor of medicine at Tubingen, to be director, and at the same time made him counsellor of state.

LXI. *Intelligence and Miscellaneous Articles.*

SNAKE OR ADDER FOUND IN A BLOCK OF COAL.

In a recent number of the Philosophical Magazine we gave a communication on the singular circumstance of two lizards having been discovered in a chalk-bed in Suffolk, sixty feet below the surface. The publication of this fact has given rise to the following affidavit of a similar discovery by two pitmen in the county of Stafford. "We, William Mills and John Fisher, both of the parish of Tipton in the county of Stafford, do hereby certify and declare, that a few years ago in working in a certain coalpit belonging to the Right Honourable Viscount Dudley and Ward, at what is called the Pieces in the parish of Tipton aforesaid, and on cleaving or breaking the stratum of coal called the stone coal,. which is about four feet thick, and in that situation lies about fifty yards from the earth's surface—we discovered a living reptile of the snake or adder kind, lying coiled up, imbedded in a small hollow cell within the said solid coal, which might be about 20 tons in weight. The reptile when discovered visibly moved, and soon afterwards crept out of the hole; but did not live longer than ten minutes on being exposed to the air, when it naturally died, not having been at all hurt by the cleaving of the coal, whose thickness and solidity must have kept it before from all air. The hollow in which it lay was split or cloven in two by means of an iron wedge; and was rather moist at the bottom, but had no visible water. It was nearly the size of a common tea-saucer; and the reptile was about nine inches long, of a darkish ashy colour, and a little speckled. After it was dead it was thrown aside; and the large coal in which it lay, being

broken to pieces, was drawn up out of the pit, and disposed of in the usual way.

"In testimony of these facts we have certified the same upon oath before the Rev. Dr. Booker, a magistrate, this 5th day of March 1817. Witness our hands,

(Signed) WILLIAM MILLS.
The × mark of JOHN FISHER.

In the presence of WILLIAM SUMMERS."

*** Properly authenticated cases of similar discoveries will always be recorded with pleasure in our pages; and those who are alive to the interest excited by such communications are requested to communicate them as often as they may come to their knowledge.

STEAM ENGINES IN CORNWALL.

By Messrs. Lean's monthly report of work done by engines in Cornwall, it appears that in February the average of 25 engines was 22,576,053 pounds of water lifted one foot high with each bushel of coals consumed.

Woolf's engine at Wheal Abraham loaded 14·9 per square inch in the engine cylinder, lifted, during the same month, 43,087,837 pounds with each bushel; and his other engine at the same mine, loaded 3·1 per inch, lifted 21,274,931 pounds. His engine at Wheal Vor, loaded 15·3, lifted 37,354,537 pounds; his engine at Wheal Unity, loaded 6·1, lifted 24,526,309; and one of his at Tadpole mine, loaded 11·5 pounds per inch, lifted 26,081,056 pounds per bushel of coals.

The Wheal Chance engine, loaded 11·2 pounds per inch, is reported to have lifted 46,961,355 pounds one foot high with each bushel in February.

PASCO SILVER MINES.

Extract from the Lima Gazette of Sept. 25, 1816.

"We have the satisfaction of communicating to the public the information, that the company for draining the mines of Pasco have just received accounts from their agents in that mineral territory, and they promise for our next gazette a description of the state of the works for fixing the remaining three engines."
—*Editor.*

"Cerro, Sept. 20, 1816.

"After having observed the progress of the machine at the Santa Rosa mine, last Saturday, the 14th instant, at ten o'clock at night, we found it began to act: at eleven the pitmen went down to clear the shaft, and have not since ceased working an instant. The clearing of the mud and rubbish which had re-

mained

mained at the bottom of the shaft, and clogged every moment the buckets and suckers of the engine, lasted till Wednesday; but this being accomplished, at twelve o'clock at noon they began to break through the level. At half a yard below the shaft we found a lively coppery ore, with its particles of silver. This bronze-coloured ore indicates that the veins of Yauricocha and San Diego mines incline to the west, or towards the Santa Rosa mine. The mines in the vicinity of this pit are all dry. Some of them, at the distance of 300 yards, in the ridge of Santa Rita, have also felt its effects; and even as far as the territory of Caya, behind our steam-works, the waters have fallen in several mines.

"Dr. John Vayas has begun to work in San Diego mine. They are also going next Monday to begin working in several points of the Santa Rosa mine. They are already eight yards in depth, and we are proceeding with the greatest activity. The workmen are relieved every two hours; and as they go out they give up their tools to those who succeed them, by which means not a minute is lost. Continuing thus, in the course of a month we shall be at more than twenty yards depth, and have many mines in full activity. The winding engine raises a basket (which is a load) in two minutes; the draining or steam-engine, with two strokes per minute, keeps the surface always dry. Both work with the greatest ease, certainty, and regularity.

"By dint of searching after a vein of coal, we have at last found one near at hand, of excellent quality, and of great richness. The pit which we now work is at the distance of a quarter of a league from Rancas, and at the same distance from Vista Alegre which the Cerro is from these works. We have likewise found a vein of plumbago, which was an object of search, on the supposition that it was coal. This substance, of which much is consumed, mixed with grease, to soften the friction of the piston, &c. we have now here, and thus the necessity of sending to Lima, or perhaps to Europe, for it, is obviated."

EXPERIMENT ON DISCOLOURED BARLEY.

To Mr. Tilloch.

"Sir,—The report in your last number, of experiments on black barley by a noble earl, whose exertions in promoting the prosperity of the agricultural interests of the nation cannot be too highly commended, deserves great consideration. But I beg leave, with much deference, to suggest, that from the various condition in which barley was last year harvested in different districts, as well as by different persons in the same district, one or two experiments ought not to be relied upon as sufficient authority for general practice. I have myself made the following:—On the 4th of February I planted, in a warm aspect, 100 kernels of black barley,

ley, taken indiscriminately from a stack harvested the second week of last October;—of these kernels 92 vegetated, and have produced roots and spiral shoots as strong as usual at this early season. From this result I would submit the propriety of every individual informing himself by actual experiment of the fitness of black barley of his own growth for seed. I am, &c.

Alderton Hall, Suffolk, March 6, 1817. J. R.

[We have thought it our duty to lay the foregoing before our readers; but on a question of so much importance, it is impossible to bestow too much caution. The Commission of the French Government, composed of the most scientific men in France, have reported, that grain which has germinated, been heated, or moulded, ought not to be employed as seed. (See their statement in page 193 of this Number.) While alluding to this Report, we cannot refrain from calling the attention of our readers to the facts therein stated respecting the making of bread from damaged grain, and to the interesting papers by Mr. Davy on the same subject, also given in the present Number.]

THE EBBING AND FLOWING STREAM IN THE HARBOUR OF BRIDLINGTON, YORKSHIRE.

About six years ago Mr. Rennie, civil engineer, recommended that a stratum of clay in the quay of Bridlington should be examined by boring, to ascertain its depth, with a view to forward some improvements then in contemplation for that harbour. This explorement was begun under the inspection of Mr. Milne, collector of the customs; and the result was, besides a perfect knowledge of the different strata, the discovery of a most singular stream of pure, soft, and limpid water, which regularly ebbs and flows twice a-day with the ocean; recedes totally with each reflux of the tide; is propelled with some force as this advances; and is evidenly more agitated during a storm, particularly at the increscent period of the tide.

These and other peculiarities, with the whole history of the discovery, are already before the public. They were communicated by Dr. Storer of Nottingham to the Right Hon. Sir Joseph Banks, P.R.S. These may be seen in the Transactions of the Royal Society for 1815, part I. and in the Philosophical Magazine, vol. xlv. p. 66 and 432.

This curious spring has been found to possess many excellent properties; and it has been administered in numberless cases of chronic disease, with decided benefit to the constitution of invalids. The water has been lately analysed at the request of a gentleman of great respectability, who, from motives of general advantage, has published the analysis, with the intention, we believe, of its being distributed gratuitously, chiefly amongst his friends. We

We have not yet seen the report of this analysis, which, we find, is from Mr. Hume of Long Acre, to whom samples of the water were sent from Yorkshire. The general result, however, we can now lay before our readers. He found, that great *purity* is one of its most distinguishing properties; that as far as the incomplete analysis of the Malverne Well, which has long been published, will allow of a comparison, the Bridlington stream is not less pure; that although this stream is so nearly connected with the sea, under which its whole vicinity is placed twice a-day, yet it is altogether exempt from muriate of soda, from any kind of sulphate, and from magnesia. According to Mr. Hume's experiments, this ebbing and flowing stream is very little heavier than distilled water. It contains no other aëriform substance than carbonic acid. The solid contents of a wine-gallon amount to 13 grains and a fraction; and these consist of

Carbonate of lime	9.625
Muriate of lime	3.750
Silex and a little oxide of iron, about	·125

ARCHITECTURE.

It is a fact deserving the notice of architects, that the resistance of any species of stone is not in proportion to its gravity. M. Gauthey has made a variety of experiments on the subject; and the most remarkable contrast which he observed is between a sort of stone dug at Caserta in Italy, and white freestone. The former of these is heavier than the latter, and yet it is found only to support half the weight. The following is the resistance of certain species given in round numbers, their specific gravities diminishing.

				Resistance.
Basaltes of Auvergne	..	2·884	..	51,945
Caserta stone ..	..	2·718	..	14,865
White statuary marble	..	2·695	..	8,176
White freestone ..	..	2·476	..	23,086
Pumice-stone ..	..	0·538	..	54,945

AFRICAN EXPEDITION.

The journal of Captain Tuckey, which is now in the course of publication, does not, we understand, hold out the least encouragement to prosecute the researches into that part of Africa which he visited. Beyond the determination of a geographical problem, there is not a single benefit, it is said, to be derived. The country does not produce any thing of advantage to a European merchant. The inhabitants, who are represented as of the lowest scale of human beings, have nothing to offer in exchange. The soil is hard and sterile: from the river Congo to

the

the extremity of the progress into the interior, a distance of 30 miles, it was observed that the ravines only were covered with a thick mould; the rest of the ground was rocky and full of stones. The conjecture formed was, that there is a junction of the two rivers, the Congo and Niger; but from the number of cataracts and rapids which occur in the course of the Congo, such a junction could be of no avail in a navigable point of view. The scientific gentlemen, it is added, employed in the expedition, felt no interest in exploring this desert region, beyond what arose from the mere circumstance of their treading upon ground which till then had never been trod by any European.

It is with extreme regret we have to add, that intelligence has just been received that Major Peddie, who commanded the other expedition intended to penetrate from Senegal through the deserts to the banks of the Niger, has also fallen a victim to his spirit of enterprise. He died before he had reached the banks of the river, and was succeeded in the command by Lieut. Campbell, who, we understand, proceeded to carry into execution the object of the expedition.

VACCINATION.

(From a Correspondent.)—Mr. Pettigrew, in his Memoirs of the Life and Writings of the late Dr. Lettsom (vol. i. page 121), says, "It was Dr. L. who first sent the vaccine lymph across the Atlantic, and consigned it to the fostering care of Dr. Waterhouse, Professor of the theory and practice of medicine in the University of Cambridge, Massachusetts, from whence *it spread* through the United States." This was stated by Dr. W. in his Treatise on the Variola Vaccina, 8vo, Cambridge, 1802. But the incorrectness of this assertion was clearly pointed out by Dr. Rhodes, in the Phil. Mag. vol. xvi. page 252, where the following passage occurs: "In the winter of the year 1799, Dr. John Chichester, a practitioner of the first distinction in Charleston, South Carolina, and to whom I have been pupil, received vaccine matter from his learned friend and former teacher, Dr. Pearson, accompanying the first publications written on the cow-pock by Dr. Jenner and himself. With this matter several persons were inoculated, but the disease was produced in one case only. This was a mulatto boy named Robert, about seven or eight years old, the property of Thomas Tunno, esq. merchant. The small-pox matter was subsequently inserted in the most careful manner without effect. It was some time after the occurrence of the above case before those which have been published as the first instances in America really happened."

Every fact connected with this great discovery will be considered of importance by posterity; and when its advantages to the

the New World shall in after ages inspire in its citizens feelings of gratitude towards the discoverer, the names of Chichester and Pearson should not be altogether forgot.

March 8, 1817. PLAY-FAIR.

[While on this subject, we may add one other fact, a recent occurrence, which will probably fill no unimportant place in the history of Vaccination :—In the electorate of Hesse a decree has just been promulgated, by which every father of a family is declared liable to a fine for each of his children who shall have attained the age of one year without being vaccinated. The fine is to be from one to eight crowns for the first year; and to augment progressively with the age of the child not vaccinated.]

SHIP PUMPS.

The following simple and ingenious method of working a ship's pumps, when the crew are either too few in number, or too much exhausted to attend to that duty when its performance is most necessary, namely, in a heavy gale, was put in practice with great success by Captain Leslie of the ship George and Susan, on a late voyage from Stockholm to North America. He fixed a spar aloft, one end of which was ten or twelve feet above the top of his pumps, and the other projected over the stern. To each end he affixed a block or pulley. He then fastened a rope to the spears of the pump; and, after passing it through both pulleys along the spar, dropped it into the sea astern. To the rope he fastened a cask of 110 gallons measurement, and containing 60 or 70 gallons of water. This cask answered as a balance weight; and every motion of the ship from the roll of the sea made the machinery work. When the stem of the ship descended, or when a sea or any agitation of the water raised the cask, the pump spears descended; and the contrary motions of the ship raised the spears when the water flowed out. The ship was cleared in this way in four hours; and the crew, of course, were greatly relieved.

STEAM-BOATS.

Mr. W. K. Northall, F.A.S., of Salop-House, Wolverhampton, announces that he has discovered a method of propelling boats by steam in a manner superior to every thing that has yet appeared. The velocity of the boat may by this plan be easily increased from three to seven miles an hour. The weight of the machinery will not be more than three tons; the space it will occupy is comparatively small. An experiment was made on Thursday the 20th of March, with a model of a boat and the appropriate machinery, in the presence of a number of competent judges, and the result is said to have been most satisfactory.

A CAUTION.

A CAUTION.

The following accident happened at Munich on the 12th of February :—An apothecary's shopman being engaged in beating up, in a mortar of serpentine stone, a mixture of oxymuriate of potash, sulphur, sugar, and cinnabar, for the purpose of making chemical matches, a terrible explosion took place, which killed the person who was making the mixture, wounded the apothecary, who at that instant entered, blew the mortar to pieces, and damaged the stove and furniture of the room.

BOTANY.

The *Agave Americana muricata*, or great American Aloe, is now in bloom at Lockinge Park, near Wantage, Berks. It is thought to be one of the finest plants of that description that has ever blown in this country. The height of the flower-stem is 21 feet. It was placed in the green-house by Charles Wymondesole, esq. about a hundred years ago.

EDINBURGH RAIN-GAUGES.

1816.	Nelson's Monument.	Observatory.	Observatory Garden.	Nursery, Leith Walk.
January . . .	0·982	0·931	2·129	2·036
February . . .	0·617	0·529	1·018	1·010
March	0·556	0·381	0·975	1·073
April	0·949	0·752	1·329	1·266
May	1·749	1·488	2·321	2·185
June	1·548	1·395	1·909	1·713
July	4·081	3·270	5·225	4·485
August	1·946	1·738	2·256	2·289
September .	2·371	2·159	2·963	2·735
October . . .	1·560	1·448	1·944	1·873
November . .	0·613	0·449	0·951	0·918
December . .	1·185	0·872	2·432	2·245
Total . . .	18·157	15·412	25·452	23·828
1815	15·412	13·115	21·830	20·617
1814	15·176	12·598	21·610	——

The rain-gauge on the top of the flag-staff of Nelson's Monument is 484 feet above the medium level of the sea, and 130 feet above the base of the monument. The rain-gauge on the tower of the Observatory is 5½ feet above the highest part of the building, and 377 feet above the level of the sea. Mr. Adie, optician in Edinburgh, made these two gauges as nearly alike as possible.

possible. The mouth of the gauge in the Observatory garden on the Caltonhill is 18 inches above the surface of the ground, and 338 feet above the level of the sea. The differences in the quantities of rain received by these four gauges arise chiefly, if not entirely, from the *wind;* the stronger the wind, the greater the differences, and *vice versâ.*

LECTURES.

Dr. Merriman and Dr. Ley will recommence their Lectures on Midwifery and the Diseases of Women and Children at the Middlesex Hospital, on Monday, April 21, at Half-past Ten o'clock.

Dr. Spurzheim has just given a Course of Lectures on the Physiology of the Brain to a Class of Medical Students and others in the City of London.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Henry Osborn of Bordesley, in the parish of Aston, county of Warwick, for his new method or principle of producing cylinders of various descriptions.—1st March, 1817.—6 months.

To Daniel Wilson of Dublin, for his gas-light apparatus, processes, and philosophical instruments.—1st March.—6 months.

To Urbanus Sartoris of Winchester-street, London, for his improvements in the construction and use of fire-arms.—11th March.—6 months.

To William Raybould of Goswell-street, Middlesex, for his improvement applicable to fire-stoves, grates, and ranges of different descriptions.—11th March.—6 months.

To Ludwig Granholm of Foster-lane, in the city of London, (captain in the royal navy of Sweden), for his new or improved process, mean or means, for preserving vegetable and animal products.—11th March.—6 months.

To William Panter of Hampton Hill, Bath, for his improvement calculated to facilitate rotatory motion, and lessen or remove friction in wheel-carriages and machinery of different descriptions.—11th March.—4 months.

To John Winter the younger, of the city of Bristol, for his method of joining and combining *horn* and tortoise-shell together, by means of heat and pressure, thereby causing the same to adhere the one to the other, in such manner as to have the appearance of solid tortoiseshell, and with all the strength and elasticity of horn, by which he will be enabled to manufacture and vend the several articles of hair-combs, ornamental and other combs, and snuff-boxes made of those materials, at a cheap rate, and resembling and having the appearance and beauty of real tortoiseshell.—18th March.—2 months.

METEORO-

METEOROLOGY.

Mock Moon.—At eleven o'clock P.M. on Monday the 24th of March, a luminous paraselene, or mock moon, was observed at Clapton by Mr. Thomas Forster. It appeared at about 25 degrees from the moon, in the northern side, and at the same elevation above the horizon. An oblong band of light extended from it. It lasted about five minutes, and was produced by the refraction of the light through a fine veil of the wane cloud or *cirrostratus*.

Aurora Borealis.

Observations on the recent appearance of the aurora borealis in southern latitudes were stated in our last to have been made in France as well as in this country. The following is an interesting account of its appearance at Paris on the same day as it was observed here, (the 8th of February,) and about the same hour, nine in the evening.

"The sky was clear, and there were seen about the zenith spots of a white hue, like the tail of a comet. A bright light like that of the dawn appeared in the north. On the quays, the bridges, and all open places, the crowd stopped to admire this rare phænomenon. But to enjoy the sight in all its sublimity it was necessary to ascend the platform of some lofty edifice. At such an elevation, where one commanded the horizon, there was beheld a dazzling circle of several degrees in breadth, extending from east to west. In the interior of this circle the obscurity was complete; and the stars of the smallest magnitude could be perceived. From the exterior part there darted from time to time innumerable beams of a white and faintly tinged light; which joining at the zenith, and even at the south, formed immense sheets, and shrunk together rapidly like a fisherman's net. The beams which issued from the extremity of the arch, at first yellowish and then orange-coloured, soon became of a deep red; and the constellation of the Bear, hardly visible through this terrible brightness, seemed bathed in blood. The sky became at last covered with clouds, and the phænomenon disappeared."

This remarkable aurora was observed on the same evening throughout the south of Germany. The appearances seem to have been on the whole much the same as at Paris, only in some places fainter than in others. Mr. Stark at Augsburg observed, that Volta's electrometer and the magnetic needle were almost constantly in motion. At half-past eight the western variation of the needle increased 1° 30′: in a quarter of an hour its oscillations were quicker; and at 18 minutes past nine its increased western variation was 2° 7′. At a quarter before ten the needle became more steady, and returned slowly to 18° 29′, where it was before the phænomenon.

Astro-

Astronomical Phænomena, April 1817.

D. H. M.

1. 0. 0 ♄ 54 ♒ nearly in contact
1. 0. 0 ♂ 45 ♑ * 14′ N
1. 4.26 ☽ γ ♍
1. 0. 0 ☽ perigee
3. 0. 0 ♀ { passes through the Pleiades, perhaps eclipsing Asterope
3.11.28 I } of α ♎ * 15 S of ☽
3.11.53 E } cent.
4. 7.56 ☽ κ ♎
4.12.23 ☽ λ
5.10.22 ☽ ♃
10. 9.51 ☽ ε ♑
11.15.10 ☽ ♄
12. 0. 0 ♄ 57 ♒ * 10′ N.
12. 0. 0 ♂ 42 ♑ * 38′ S.

D. H. M.

12. 0. 0 ♂ { 54 ♑ nearly in contact
13. 0. 0 ♄ 58 ♒ * 8′ S.
16. 0. 0 ☽ apogee
17. 0. 0 ♂ 57 σ ♒ * 13′ N.
17. 0. 0 ♂ 58 ♒ * 6′ S.
18. 0. 0 ♂ ♄ nearly in contact
19.12.17 ☾ ♀
19.23.28 ☉ enters ♉
20. 0. 0 ♂ 64 ♒ * 9′ N.
23. 3.13 ☽ κ ♊
24. 4.56 ☽ γ ♋
25.17. 6 ☽ η ♌
27.13.20 ☽ ν ♍
28.15.14 ☽ γ ♍

Meteorological Observations kept at Walthamstow, Essex, from February 15 *to March* 12, 1817.

[Usually between the Hours of Seven and Nine A.M.]

Date.	Therm.	Barom.	Wind.
February			
15	40	29·53	S.NW.—Rain; foggy; fine sunny day; bright starlight.
16	41	29·61	NW.—Sun through clouds; fine day; windy; star-light. Full moon.
17	48	29·81	S.—Foggy; windy; cloudy; fine day; cloudy.
18	48	30·51	SW.W.—Foggy; damp day; slight rain; star-light.
19	40	30·51	NW.—Sun and hazy; very fine day; star-light.
20	44	29·93	S.—Cloudy; rain; sun through clouds; slight rain; moon and star-light, and windy; *aurora borealis* NE. 10 P.M.
21	33	29·54	W.—Clear and clouds; *cirrostratus* and wind; sun and wind; great showers; wind and stars.
22	42	29·61	NW.—Sun and wind; some *cirrostratus* NW; sun and wind; clouds; moon and star-light. *Corona* round the moon.
23	38	29·93	W.SW.—Foggy; cloudy and windy; rain and wind.
24	42	29·77	NW.—Clear, and clouds, and wind; cloudy day; cold; light, but neither moon nor stars visible. First quarter.

February

February			
25	42	29·99	W.—Hazy; fine gray day; bright moon-light 10 P.M.
26	40	29·83	NW.—Showers; wind and sun; very fine day; cloudy.
27	47	29·69	W.—Wind; sun, and *cirrostratus*; great showers, and sun; moon and stars and *aurora borealis* N.W.; *high* and changeable, and *not* steady.
28	47	29·90	W.—Clear and clouds; fine day; sun and wind; a slight shower; light, but no moon nor stars visible.
March			
1	50	29·72	S.—Foggy; some sun through clouds; gray day; wind and hazy.
2	40	29·71	SW.—Sun and hazy; fine day; windy; a slight shower; rain and wind.
3	38	29·43	NW.—Wind and sun; rainy afternoon; rain and wind; *stormy* and *cumuli*. Full moon.
4	39	29·22	W.—Clear sunshine; fine day; some showers; bright moon-light, and windy.
5	32	29·22	W.SW.—White frost; fine cold sunny windy day, and drops of rain; moon and star-light.
6	39	29·83	SW.NW.—Showers; sun and wind; great wind and showers; fine afternoon; bright star-light.
7	32	29·22	W.NW.—Clear sun; white frost; fine cold day; very dark night.
8	33	29·01	N.—Snowing at 7 A.M.; then rain; at noon sun and showers of rain; 7 P.M. bright star-light; 9 dark and rainy.
9	37	29·43	N.—Clear and *cirrostratus*, and very windy; sun; clouds, and slight showers; bright star-light.
10	42	29·90	NW.—Clear and *cirrostratus* or wanecloud; sun; clouds, and wind; fine day; star-light. Moon last quarter.
11	44	30·21	N.—Hazy all day; dark night.
12	44	29·99	S.—Hazy; *stratus* N.W.; fine day; very dark night.

N. B. *Errata in last month's Magazine.*—9th February, for *cloudy, clear*, read *steady clear.*

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1817.	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Feb. 14	28	42·	29.60	Fair—gale from W
15	29	48·	29 53	Ditto—heavy rain at night
16	new	44·5	29·80	Fine—blows hard
17	1	54·	29·96	Cloudy
18	2	50·	30·06	Ditto
19	3	45·	30·28	Very fine
20	4	51·	29·67	Cloudy
21	5	42·	29·51	Fine—blows hard from W.—snow and rain at night
22	6	47·	29·77	Ditto ditto
23	7	47·	29·90	Cloudy ditto SW
24	8	46·	29·80	Ditto ditto
25	9	51·	29·99	Ditto ditto
26	10	44·	29·80	Stormy ditto—hail—squally
27	11	47·5	29·65	Fair—violent gale W
28	12	49·5	29·84	Showery
Mar. 1	13	52·5	29·68	Fair
2	14	48·	29·51	Ditto— blows hard W.—showery P.M.
3	full	44·5	29·32	Cloudy—heavy rain at night
4	16	41·	29·20	Ditto
5	17	45·	29·37	Fair—blows hard W
6	18	44·	29·02	Fair—stormy P.M.
7	19	44·5	29·35	Ditto ditto—frost at night
8	20	40·	29·26	Hail storm A.M.
9	21	44·	29 60	Very fine—gale from NW
10	22	43·	30·15	Ditto
11	23	51·	30·20	Fair—showery A.M.—frost at night
12	24	54·5	29·99	Ditto ditto
13	25·	55·	30·05	Cloudy ditto
14	26	48·5	30·35	Very fine—wind changed to E at 3 P.M.—frost at night
15	27	46·	30·35	Fair—cold fog P.M.

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For March 1817.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Feb. 27	47	50	43	29·60	37	Fair
28	46	54	47	·80	37	Fair
March 1	47	53	45	·60	23	Cloudy
2	39	47	42	·70	31	Fair
3	38	44	43	·10	0	Rain
4	41	45	37	·17	35	Fair
5	35	46	39	·25	34	Fair
6	37	45	40	28·90	26	Fair
7	33	47	41	29·25	46	Fair
8	36	45	36	·17	32	Snow showers
9	35	44	36	·40	24	Fair
10	35	47	35	·91	34	Fair
11	36	50	46	30·05	18	Cloudy
12	46	51	50	29·85	22	Cloudy
13	50	55	46	·87	33	Cloudy
14	46	50	40	30·10	37	Fair
15	40	51	40	·10	29	Cloudy
16	39	47	37	·09	35	Cloudy
17	35	47	36	·17	27	Fair
18	35	55	43	·18	42	Fair
19	45	45	36	29.72	40	Fair
20	33	37	26	·71	46	Fair
21	27	42	27	·80	45	Fair
22	26	44	32	·81	40	Fair
23	32	44	42	·90	36	Fair
24	42	53	45	·79	25	Cloudy
25	46	55	40	·72	32	Showery
26	45	54	39	·70	22	Cloudy

N.B. The Barometer's height is taken at one o'clock.

LXII. *Remarks on a Paper by Mr.* Dalton *on the Chemical Compounds of Azote and Oxygen, &c.* *By* William Higgins, *Esq.*

To Mr. Tilloch.

Sir, — You will oblige me by inserting in your candid and useful Magazine the following remarks on a paper of Mr. Dalton on the Chemical Compounds of Azote and Oxygen, &c. which appeared in the number of Dr. Thomson's Annals of Philosophy for February.

It is stated that this paper was read before the Literary and Philosophical Society of Manchester in the year 1816. That it should lie by since is not to be wondered at, as containing nothing new; it relates to a hackneyed subject, which chemists have lately gone over repeatedly.

In that part of my paper which appeared in your excellent Magazine for December 1816, I observed that Dr. Thomson stepped forward repeatedly in a very unjust cause, which could never do him credit, as the advocate of Mr. Dalton, while the latter stood silent and trembling at the bar of justice.

In consequence of the above observation it was, I suppose, that his friend urged him on with his tale, which is obscurely and contradictorily told; and I may add evasively and unjustly so, respecting myself. But the sole object was to say something on any part of my system with a view to show his face and a confidence of his innocence, at the same time that no mention should be made of the person whom he so glaringly attempted to injure.

I will now give a very cursory view of this paper, for more is not necessary. "Lavoisier (he says) was the first who ascertained the constituents of the atmosphere, thirty years ago."

Priestley was before Lavoisier in the discovery: but it is of no consequence, as to my present object, which of them was first or second, except in point of justice, which we should never lose sight of as men or philosophers. But Mr. Dalton goes on, "Lavoisier in consequence of vague and contradictory expressions was not decided whether the oxygen and azote of the atmosphere were mixed only, or chemically united; and as in his table of binary combinations of azote with simple substances no mention is made of atmospheric air being one of them, it is likely he considered it as a simple mixture."

This is puerile: there is no chemical combination of azote and oxygen in the proportion in which they exist in our atmosphere; and were they chemically united, it would be unfit to support animal life.

Chaptal comes next. "He was decisively of opinion that our

our atmosphere consisted of a mixture of oxygen and azote. Sir H. Davy considered the constituents of the atmosphere as chemically united; and his reasons for supposing so are adduced, but must be relinquished as ill founded, &c."

Mr. Dalton next alludes to his Essays on the Constitution of mixed Gases, published in 1802; containing an hypothesis to explain the uniform diffusion of gases by mechanical means. On this principle, the atmosphere was considered a mixture not a combination of its elements.

Before I proceed any further with Mr. Dalton's paper, I will in a few words mention the prominent features of his hypothesis. He supposes that one gas affords a vacuum in its interstices for a second gas of a different nature from itself, and that each particle of the same gas in the mixture presses only on its kindred particle; so that the pressure of each of the different gases which constitute our atmosphere is independent of the other; not one of them pressing on the other, but all distinctly with one uniform weight rest on the surface of the earth. The same law holds good as to the mixture of other gases.

This is a strange hypothesis, which one cannot well reconcile to common sense. All kinds of gases mix uniformly, and there are many gases whose ultimate divisions are of the same size, and some gases whose ultimate particles are much smaller than others; yet their atmospheres of caloric are often larger. The calorific atmospheres of hydrogen are larger than those of oxygen. There are some gases whose atmospheres are of the same size;—how then can all gases indiscriminately afford vacant interstices one for the other?

Suppose two parts of one gas were mixed with twenty parts of another gas,—how could the particles of the small quantity of gas extend their pressure to each other *only*, when such a number of the particles of the predominating gas must be in their way? It appears to me more reasonable to suppose that under those circumstances the different kinds of particles or their atmospheres, which is the same in effect, must rest indiscriminately on each other.

Many objections have been made to this hypothesis, and many more might be brought forward were it considered necessary.

I do not make these cursory remarks with a view to completely invalidate this hypothesis, for they are not sufficient to accomplish such an object. My reason for taking any notice of those fanciful conceptions will appear presently. I must say that it is the only part of his New *System of Chemical Philosophy* that he has not borrowed; for the principal and most interesting part was taken from my *Comparative View*, as had been clearly proved by many, and lately confirmed by my own writings.

But

But to return to the subject of atmospheric air and its gases.—"Soon after this appeared (meaning his hypothesis of mixed gases) Berthollet, in his Researches into the Laws of Chemical Affinity, announced a new explanation on the phænomena of mixed gases. According to this eminent chemist, there are two species of affinity; the one strong, the other weak:—the strong affinity makes bodies unite chemically; the weak only serves to diffuse them through each other without producing condensation of volume; its effects may be called solution or dissolution. Of this kind, he conceives, is the mutual action of gases that do not combine, and that it operates just the same upon gases inclined to combination or not:—thus a mixture of carbonic acid gas and hydrogen is subject to this weak or slight affinity just as much as one of oxygen and hydrogen. Something similar to this is supported by Murray in his Elements of Chemistry, and by Dr. Thomson in the third edition of his Chemistry. Mr. Gough in different papers in the Manchester Memoirs, and in some essays of his in Nicholson's Journal, endeavoured to support the opinion of atmospheric air being a chemical compound; but he does not avail himself of the two affinities of the strong and the weak, in order to explain the phænomena of mixed gases." Here ends Mr. Dalton's old detail; and had he given a true history one might probably read it without disgust.

The distinction of weak and strong affinity is of considerable importance in Nature, and they should be well understood and defined from each other. What is called weak affinity does not only prevail between permanent gases, but also between those gases and the vapour of water and all other vapours, and even between fluids and solid substances.

I will now prove that I was the first who made this important distinction between weak and strong affinity twenty-eight years ago. At that distant period I felt doubtful whether the sulphur and hydrogen in sulphuretted hydrogen gas were chemically united or not, from the great facility with which they were separated in a variety of ways. This consideration led to the following remarks, which I quote from page 73 of my *Comparative View*.

"In my opinion it is mere solution (that is, that the sulphur is held in solution by the hydrogen), such as takes place between the neutral salts and water, the alkalies and water, and sugar and water, &c. Although the facility with which sulphuretted hydrogen is decomposed favours this hypothesis, yet there are circumstances apparently against it, particularly its condensation in water, and its expulsion from it again by heat without decomposition.

"Upon what principle this modification of attraction exists

between

between bodies, has not been explained ; and the difference between it and chemical union has never been defined or discriminated by chemists.

"It appears to me that solution, or that power by which water dissolves or condenses carbonic acid gas, pure ammoniacal gas, sulphureous acid gas ; and that power by which hydrogen dissolves sulphur and phosphorus ; and also that power by which all the gases dissolve water ; and *lastly*, that by which water dissolves saline bodies, &c. without changing their nature or properties, is occasioned by a sort of intermediate attraction, not differing much from chemical influence but in its degrees of force, and not at all different from that power whereby the planets influence each other."

There was an idea entertained about this time, that many fiery meteors were occasioned by the collection and inflammation afterwards by electricity of hydrogen in the upper regions of the atmosphere. This induced me to make the following experiment. I made a mixture of two parts of hydrogen and one of oxygen by measure. With this I filled a jar eighteen inches long and nearly three inches in diameter. This mixture, after having stood in dry quicksilver for nine months, was found to be uniform throughout the whole column ; for when a small portion of the lower part was transferred with little or no agitation, it was wholly condensed into water by the electric spark.

The rotundity of a bubble of air, whether simple or mixed, shows a strong influence of its atoms or particles on each other : and probably this influence, as I originally supposed, is occasioned by the gravitation of their solid particles towards each other, particularly as those particles must be in proportion to their diameters removed a considerable distance from each other.

It is remarkable that Mr. Dalton should enumerate the slightest and the most trifling observations of different authors who wrote after me on this subject, and pass over the facts above quoted which must be allowed to be original. He cannot plead as an excuse that he had not read my *Comparative View*, after what has lately occurred—No, the omission is evidently wilful. I could expect nothing else.

It is said that there is a species of depravity peculiar to human nature—which is, that we hate the person we injure more than any other individual. This is readily accounted for.

The rest of this paper relates to the different chemical combinations with azote and oxygen according to the different proportions in which they unite. He gives the opinion and experiments of a great many chemists, who have attempted to prove the quantity of azote and oxygen by weight and measure in the different compounds which those elements are capable of forming.

No

No two of them agree, and he differs a *little* from them all: but he takes care to adhere to the middle line of difference; for any person intimately acquainted with experimental chemistry will readily perceive, in reading his *New System* of Philosophy, that he is not an experimenter, notwithstanding what he asserts on that subject: in short, whatever knowledge he possesses is evidently derived from reading the experiments and the writings of others.

I will therefore pass over this contradictory detail, particularly as it is almost in substance what Dr. Thomson advanced in his History of the Atomic Theory in the *Encyclopædia Britannica*, of which I have already taken notice in this Magazine for November and December 1816.

The only part of it that deserves any attention is what relates to the doctrine of definite proportions, which *Mr. Dalton* attempts to explain in his own way; but no mention is made of its original author. On this part he begins thus: "The subject of the greatest difference amongst us is in regard to the absolute weights of the elements azote and oxygen, which combine to form the several compounds. Gay Lussac and most of the other chemists I have mentioned, who follow him as volumists*, contend that the proportions are as under: viz.

Measures	*Measures.*	*Measures.*
100 azote	+ 50 oxygen	=100 nitrous oxide.
100	+100	=200 nitrous gas.
100	+150	= subnitrous acid.
100	+200	=100 nitrous acid gas.
100	+250	= nitric acid.

The foregoing proportions are pretty correct, as shall be shown presently.

"But (continues Mr. Dalton) from the views I entertain on the subject as derived from experiments, the true portions of

* My *Comparative View* was published twenty years before Mr. Dalton's *New System of Philosophy* appeared, and Gay Lussac had written some time after him. It will be found by any person who will take the pains of carefully perusing my work, that I was perfectly acquainted with the proportions in which gases combine in volumes; it was the ground-work, together with their specific gravities, on which my entire system rested: and without this knowledge no human being could attempt to estimate the different proportions in which they unite particle to particle; and much less the relative size, and of course the relative weight, of those particles; for the specific weight of the ultimate divisions of all kinds of ponderable matter is the same—their size or diameter only constitutes the difference.

Without a previous knowledge of the foregoing principles, we might as well attempt to ascertain the weight of the most distant fixed stars, as those of particles, atoms, or molecules of matter: hence it is evident that the above passage of his operates more against Mr. Dalton than any thing that could be brought forward.

 the

the compounds would be more nearly stated as under." Then his table of measures of the proportions in which the two gases unite, and symbols to represent their atoms, are given. I shall content myself at present by describing those proportions of atoms and measures, beginning with the nitrous oxide, as containing the smallest proportion of oxygen.

Mr. Dalton, wishing to differ from every body else, states that 100 of azote and 60 measures of oxygen enter into the constitution of nitrous oxide; and he represents the compound atom as consisting of two ultimate particles of azote and one ultimate particle of oxygen. Now it has been shown by Sir H. Davy and by myself, in a variety of ways, a good many years ago, that this gas consists of one volume of azote and half a volume of oxygen; and as each particle of the azotic gas was supplied with a particle of oxygen, I inferred that the half volume of oxygen contained as many divisions as one volume of azote, on the same principle that one volume of oxygen contains the same number of divisions that the two volumes of hydrogen do. I also inferred that the ultimate particles of azote are nearly twice the size and of course twice the weight of those of oxygen, although the latter gas is somewhat heavier. The specific gravity of gases, as I have long since proved in my *Comparative View*, does not always indicate the real weight of their ultimate particles or atoms; and this circumstance has led chemists into many blunders, particularly those who have attempted to ascertain the weight of atoms and molecules on the principle of my definite proportions.

But to return to the gaseous oxide.—It would require 200 measures of azote in the place of 100, to supply the atoms of this gas with two ultimate particles of azote; and the ten measures of oxygen which he has thrown in, by way of showing the accuracy of his philosophy, are quite superfluous.

I consider azote, like almost all substances that unite to oxygen, as an inflammable base; and an ultimate particle of oxygen never unites to two ultimate particles of any inflammable base: while the reverse is the constant law of nature, as I particularly demonstrated throughout the whole of my *Comparative View*, and I have laid great stress lately on the importance of this law in my *Atomic Theory* and *Electrical Phænomena*.

Next in regular order comes nitrous gas.—He allows this gas 100 of azote and 124 of oxygen by measure, and its atom is represented as one of azote and one of oxygen. 100 measures of oxygen would give every ultimate particle, in 100 of azote two ultimate particles, so as to form an atom of nitrous gas; therefore, according to Mr. Dalton's atom, 74 measures of oxygen remain unaccounted for.

Agreeably

Agreeably to my experience, nitrous gas consists of one measure of azote and one of oxygen, and the latter affords two particles to every one of the former so as to constitute an atom.

When nitrous gas and nitrous oxide are mixed, they only diffuse through each other mechanically; for the nitrous gas retains its second particle of oxygen with as great force as the other gas can attract it—therefore no change can be produced. None of those gases possess an acid property.

The third compound of those elements is called subnitrous acid; it is what I distinguished in my *Comparative View* by the name red nitrous acid, from its red colour. This acid according to Mr. Dalton consists of 100 of azote and 186 of oxygen by measure, and the atom is represented by his diagram or symbol as consisting of three ultimate particles of oxygen and two of azote. Let us examine those proportions:—In the first place, 150 measures of oxygen would supply every particle in 100 of azote with three particles of oxygen; there remains therefore a surplus of 36 measures of oxygen. As to the second particle of azote, I cannot conceive whence it came or how supplied, unless indeed the atoms were cloven in two.

This acid, according to my experiments, contains one measure and a half of oxygen and one of azote; and the atom, as represented in my *Comparative View*, consists of one particle of azote and three of oxygen. It is difficult to obtain this acid pure; that is, free from the pale or straw-coloured nitrous acid which will be described presently. It is obtained in a tolerable degree of purity when nitrous gas is mixed in excess with atmospheric air, or when nitrous gas is passed into nitric acid until it is incapable of receiving any more. This acid can exist in the gaseous state when excluded from water.

The next and fourth compound is distinguished by the name of nitrous acid gas by some modern chemists. I called it in my *Comparative View*, pale or straw-coloured nitrous acid, from its colour. Mr. Dalton supposes this acid to consist of 100 of azote and 248 of oxygen by measure. The 200 alone, laying aside the 48 measures of oxygen, would supply every particle of the azote with four particles of oxygen; and yet, to my very great surprise, Mr. Dalton's symbol represents the atom of this acid as consisting of one particle of azote and two of oxygen, the proportions which the atom of nitrous gas contains.

In my *Comparative View* I represented the atom of this acid, by means of a diagram, as consisting of one particle of azote and four of oxygen.

It is very difficult to obtain this acid pure; for in the common way of distillation it comes over with more or less of red nitrous acid, or with a mixture of nitric acid. This acid is produced by

 mixing

mixing two measures of nitrous gas with one of oxygen. This shows that the one measure of oxygen contains twice as many particles as there are atoms in two measures of nitrous gas. This was one of the many facts which led me originally to the doctrine of definite proportions—that beautiful law by which oxygen unites to bodies in different doses, and that the 2d, 3d, 4th, and 5th were all distinct multiples of the first or minimum dose.

In producing the pale nitrous acid by mixing oxygen and nitrous gas, there are sometimes formed atoms of the red nitrous acid mixed with the pale, therefore less oxygen is consumed; and again, particles of nitric acid are formed, which demand more oxygen, and which only *mix* with the pale;—in these cases different quantities of oxygen are consumed by the nitrous gas. The foregoing variations depend upon the manner in which the gases are mixed, and the surface which they present to water.

These facts will account for the difference of opinion entertained by chemists respecting the proportions in which the two gases unite so as to form, according to them, the same acid.

The fifth and last combination of those elements is the nitric, which I called in my *Comparative View* the *colourless* nitrous acid, as being when perfectly pure as limpid as distilled water.

Mr. Dalton represents this as composed of 100 measures of azote and 310 of oxygen: this quantity should give more than six ultimate particles of oxygen to each particle of the azote. His symbol gives only five, with the extraordinary proportion of two of azote:—whence comes the second particle of azote is best known to Mr. Dalton himself; I cannot make it out, I must own.

In my *Comparative View* I represented the atoms of this acid as consisting of one particle of azote and five of oxygen. My definite proportions of the different compounds which those elements are capable of forming, are as follow:

Nitrous oxide	1 and 1.
Nitrous gas	1 and 2.
Red nitrous acid	1 and 3.
Pale nitrous acid	1 and 4.
Nitric acid	1 and 5.

The variation of the different forces of attraction according to the quantum of oxygen was also stated, which is the most important part of the whole system, and which alone could enable me to account for the following facts; viz.

1 and 1	will have no effect on	1 and 2.
1 and 2	will not affect ..	1 and 3.
1 and 3	no effect on	1 and 4.
1 and 4	no effect on	1 and 5.

They will only mix mechanically.

On

On the other hand, in the following order they will have a chemical action on each other, so as to produce a partial decomposition.

1 and 2 will take one portion of its oxygen from 1 and 4, and 1 and 3 will be produced between them both.

1 and 3 will also deprive 1 and 5 of one portion of its oxygen, and 1 and 4 will be the result.

The foregoing facts are founded on experiments, and they prove that my proportions are indisputably correct.

I will here make one remark on Mr. Dalton's symbols;—that is, he unites my 1 and 1 to my 1 and 2 to form an atom of red nitrous acid, and my 1 and 2 with 1 and 3 to form an atom of nitric acid. Now these atoms are incapable of uniting chemically, as I stated above.

Considering that those distinctions of the definite proportions in which azote and oxygen unite to form the foregoing different compounds originated with me, as had been often stated, and which facts and dates render as clear as possible, it is really extraordinary that Mr. Dalton should not even mention my name in treating on this small part of my system, were I ever so wrong in my calculation. But the wonder will cease when it is considered that he brings forward as his own my diagrams, which represent the proportions in which the particles of the different elements unite so as to form distinct atoms, in a mutilated and discordant state in the form of symbols, in order to disguise them from the original. But what is still worse, in those symbols of his, monstrous proportions are represented, which Nature never produced.

I am inclined to think that Mr. Dalton himself, after what has passed respecting him and me, and after three or four years silence on the subject, would never come forward with his old song so wretchedly set to music,—if such I can call his symbols,—without making some apology for what has lately passed on the subject of the atomic theory. It must be a *ruse de guerre* of one of his friends, and I strongly suspect who the person is; but as Mr. Dalton's name is to the paper, and no doubt with his consent, he alone should be addressed. It is very much to be lamented that science should be infested with such juggling.

There are at present a certain number of writers on chemistry, who, to the disgrace of the nineteenth century, make it a common practice to play into each other's hands, and to deprive men of superior talents and information of the originality of their discoveries; and should it happen that they cannot take immediate possession of them, they pass them by unnoticed in their compilations until a more favourable opportunity offers. This shameful

ful conduct must, and has already injured the cause of science: for to my own knowledge many excellent experimenters have retired from their labours in disgust. I have the same feeling; but from my situation as professor to one of the first institutions in Europe, I must continue to fight my way; not for myself so much as for the cause of science, for I have already established my original claim.

I have many to oppose, but with justice at my back I feel myself equal to them all. Perfect security of every species of property, whether it be scientific or otherwise, is thegreat spur to industry; and this sacred security once removed, farewell to all human efforts!

I am, sir,

Your very humble servant,

Dublin Society, March 22, 1817. WM. HIGGINS.

LXIII. *Answers to the Geological Queries by* N. J. WINCH, *Esq. with some Remarks, and further Queries proposed to that Gentleman, and to other practical Investigators of the Strata of the North of England. By* A CORRESPONDENT.

To Mr. Tilloch.

SIR, — I BEG to tender my best thanks to *Mr. Winch* for the new facts regarding the interesting Stratification of Northumberland, Durham, Yorkshire, and Cumberland, which he has furnished pp. 207 and 208 of your last Number; being willing and anxious to consider these, as preludes to much more numerous and precise *local facts*, in answer to my several Queries in p. 122 &c.; part of which, Mr. W. might now, perhaps, furnish from his Notes, or will ere long, from inquiry or inspection of the spots, be able, it is hoped, to furnish, and which I will still rely on his kindness to do, and hasten to return the best answers in my power, to his two queries in p. 208.

1st. I certainly consider, as I have understood Mr. Smith, Mr. Farey (P. M. xxxix. p. 256, and xliii. p. 256 &c.) and many others to do, the Alum-shale series and irregularly thin Coal-series on it, in the north-east of Yorkshire, to be included in *the assemblage of Strata*, of an argillaceous character principally, which Mr. Smith, its discoverer, found it convenient, and without any theoretic views to designate the "*Clunch Clay*;"—I decline for obvious reasons entering into the discussion of "distinct *formations*;" where "Doctors disagree," practical Men, in search of facts and useful truths only, had better keep aloof.

I will

I will however add, that in most instances, as has been done about Lyth*, those who have with effect studied the Smithian principles of Mineral Surveying, will be at no loss to subdivide Mr. Smith's "clunch clay," into several Strata, and distinctly show their continuous ranges on a Map of sufficient scale, with Sections in any direction, &c.; and by means chiefly of the fossil Shells *species*, rather than by the precise Mineral *species* (by which "formations" are usually and solely attempted to be identified) to *identify the same* (as Mr. Smith has done) in situations too distant, or which are too much intersected by *faults*, or by alluvial or *unconformably* overlieing Marl patches, for each stratum to be actually traced in connection on the surface. See my paper on Fossil Shells, vol. xlv. p. 274.

2d. I have never visited Thirkelby, or made any observations myself, in the small Coal-field to the E and SE of Thirsk; but from various notes of the inquiries I have made concerning the strata of the vicinity of Thirkelby, I can entertain few doubts, of the blue shelly Limestone of that place, being one of several thin Rocks of shelly Limestone†, which lie between the upper and better part of the Alum-shale and the thick, harder and useless lower part of that series; which last would more properly be denominated Bind with Stone beds, than Alum-shale: I have seen these Limestones, at the SE corner of Staith's Bay NW of Whitby, from the top of the cliffs over Boulby Tunnel, on the W of Staithsbeck Fulling-mill, at Blue-Bank, and in other cliffs by the Esk River, and I can entertain no doubt that I should be able to trace these, wherever alluvia or unconformable Red Marl is absent or deranging faults do not intervene, in a continued course, to the vicinity of Thirkelby, and thence forward, into and across the eastern part of Lincolnshire.

I heartily wish that Mr. Buckland or Mr. Fryer, the latter Gentleman in particular, because he is locally resident, and has at this time the whole of Northumberland and Cumberland under his Survey, for publishing improved Maps of these Counties, would carefully, and without reference to any Theories, consider my five heads of queries in your February number, and those which now follow in continuation, and communicate through your pages, all the precise facts they can, which have reference to them.

* The Writer thinks it probable that Mr. Winch might, on application to John M. Sowerby, Esq of Lyth Hall, have access to a Mineral Map and Report on this district, made in 1811 by Mr. Farey, which would somewhat assist his inquiries on this head.

† It has often struck me, but leisure has never permitted of *comparing the shells*, that these shelly Limestones, answer to the higher beds of the *Bedford* or Cornbrash Limestone Series, of Smith's Map.

6. In

6th. In what direction and degree do the Coal-measures of Tindal-fall *dip?*: what is the series sunk through in their Pits? ; are there in the vicinity either eastward or westward, any considerable masses or strata of Basalt in any of its varieties (not Dykes)? ; and if so, where are they situated precisely? ; In what direction do they dip and *take-cover?*; and what are the strata bassetting from under them, in an opposite direction?.

An intelligent Gentleman, who has all his life resided in the elevated country south of Jedburgh, and who, though no professed Mineralogist, is well acquainted locally, with all "the tract due west and north-west of the Cheviots," happening to call on me a few days ago, and to whom I showed Mr. Winch's Map and description of the Cheviots, in the Geological Transactions, professed his ignorance of any coarse slaty Rocks (like those about Hawick and Selkirk which he knows well) in the vale of the Beaumont, or on Carterfell; although there are (he says) some patches, or a narrow range of such coarse slaty Rocks, along the south-eastern verge of Cheviot, although far less extensive, he believes, than the "Grauwakke slate" district, coloured there in Mr.W's Map, who has written Carter-fell in the same, instead of its true position, to the SW, facing the head of Reed Dale: and certainly this Scotch Gentleman said, the coarse slaty Rocks here, do not *join on the surface*, or near it, to those like them about Hawick. I beg therefore to inquire,

7th. Can the very lower part of the Coal, Sandstone, Shale and Limestone series, sometimes called by Mr.Winch the "Lead-mine Measures," which he represents as surrounding and lapping on the north, the east, and the southern flanks of the Cheviot mass, be in like manner traced continuously, on its western side, from Kerryburn to Mindrum? If so, by what route, exactly? ; and what are the dips, at several points, in this range?.

For the *theoretic opinions* of Mr. W. or any other persons, as to whether Basalt "owes *its origin* to *a different cause* from the *regularly stratified Rocks* with which it is associated," the Querist had not the most distant intention of asking: but his wish was, and is, for the communication of *precisely localized facts*, regarding the present *positions and circumstances of this and all others of the mineral Masses* or strata, of which the northern parts of England are composed, and for such he will ever feel obliged. He will now however, not let slip the opportunity of remarking, that after examining carefully, many score miles of the ranges of Basaltic masses, he has uniformly found them, *as distinctly stratified and interlaid between regular strata*, as most of the latter are, with regard to one another: not one anomaly as to Basaltic strata appearing, except in degree, from what is very common with other strata.

Every

Every competent Mineral Surveyor, or Person among those who properly consult practical *Sinkers, Soughers, Quarriers*, &c., and who collect, consult and contrast Specimens from the British Strata, (I pretend not to speak of countries where Mineral Surveying is unknown or unapplied) must at this day be acquainted with the facts, that all the strata, without exception, though in different degrees, are liable, if I may so express myself, to considerable *variations in their thicknesses*, as well as in the precise *mineral species* of which their masses are composed, in different parts of their course or their dip. The same stratum, often swelling into Bumps, or more commonly Ridges, and changing almost completely, but more or less suddenly, many of their external and even their chemical characters, yet *frequently returning to all these again*, or most of them, as we proceed in tracing their courses. That between the thicker and more perfect parts of very numerous strata, they are sometimes found so very thin and altered, as to be with considerable difficulty recognised; and yet, has each stratum, a pretty well marked *place in the Series*, of *successively deposited strata*; such successive strata eras, being marked, by the existence and extinction, of a series of Organized Beings: many of these *places* in the series, are now well ascertained and known, and by the zealous cooperation of such Men as Mr. Winch, in freely and promptly communicating in the pages of the Phil. Mag. a vast many more may speedily be settled, and the Geological knowledge of Britain proportionally advanced.

And with regard to *Dykes* or Stone Veins, there really appear to me, less anomalies with regard to Basalt, from Sandstone, Clay, and other mineral masses, *all which fill Dykes in vast numbers*, as all Colliers know, than with regard to strata of these substances, respectively. Without attempting the task, at present an impossible one, of assigning *the source or origin of Mineral Matter*, generally, whether composing strata or filling fissures, subsequently opened in strata previously consolidated, both seeming equally beyond our comprehension; —it seems to me perfectly plain, from a very extensive series of observations, that fissures were originally opened, some in nearly equal degrees, so as to produce almost flat and parallel cheeks or separated edges of the strata, and others of these cheeks were slightly, and some very suddenly concave, consistently with *general or local causes of shrinking* or contracting in the mass; the former of these fissures, the almost parallel sided ones, commonly *intersecting* each other (as the doubly wedging or lenticular ones sometimes do not), and rarely crossing, but more commonly ending in each other, so as to separate the mass of the

the Earth's crust into very numerous piles of strata, almost or entirely separated from each other, by these original fissures: the greater part of which, originally open fissures, have, since, been symmetrically filled up, by layers or ribs of mineral matters (of very numerous kinds in different cases) applied successively against the skirts, until the fissure was filled; producing thus, almost universally, the appearances of nearly vertical newer strata or ribs*, intersecting and dividing the original strata, almost perpendicular to their planes; some of which ribs, are nearly parallel to each other, and others curved, and forming often, very suddenly, lenticular masses, in the fissures which had previously been open. I have in no instance been able to discover clear marks, of mechanical violence having dislocated the piles of strata, *previous* to the filling up of the Dykes and veins: but *since* that well marked period in the history of our Strata, a tremendous external *force seems to have operated*, to fracture the Earth anew, and *dislocate* its fragments; such new and dislocating fractures (usually now termed *faults*) having passed along the former fissures, in most, but not every instance, the solid dyke-stuff or vein-stuff, usually forming now one side of these second or fault fissures; and such dyke-stuff exhibiting the same marks of *violent grinding*, as the adjacent edges of the fractured strata do. Into these second and *dislocating fissures* it is, that all the extraneous matters, like real *rolled* pebbles (not *nodules* formed in the veins), organic remains, &c., have found their way *from the surface*, both into Mineral Veins, and into Faults. The almost general fact, of the faults proving *water-tight*, and separating the strata, into distinct plots or drainages, arises mostly, I think, from the perfect continuity of the previous vein or Dyke-stuff and its remaining entire on one side (and in some rare instances on both sides) of the Fault, and not from the violent jamming in of soft clayey matters, into the fault: yet in many instances, where a mere crack or very thin fault-stuff is visible, and yet proves water-tight, it would seem, that some such operation has taken place, as with the Sinker, who has occasion to blast in a porous rock, full of water, and has recourse to the expedient, of cramming his hole quite full of very stiffly-tempered clay, and then very forcibly hammering the same in, by means

* I do not mean by this to be understood to assert, that any considerable portion of *all the vertical strata*, or those very highly inclined, which are met with, occupy Dykes: because in general, such are occasioned by the strata having been applied on the inclining or almost vertical slope of *a bump* or ridge in a previously deposited stratum For it is by no means necessary or true, that all strata, even the most shelly or conglomerate, must have been deposited in an *horizontal* position: as short-sighted theory has asserted.

of

of a plug, nearly fitting the hole, by which operation the pores of the stone are so closely plugged up, that when the Clay is again bored out, the hole will remain free of water, for receiving the charge of gunpowder in a proper cartridge.

But I have been unintentionally led into a long digression, which I hope Mr. Winch's goodness will excuse and permit me to return, and mention, that I conceive we should not "naturally consider," that basaltic eminences are anything else but the thicker parts of a continuous stratum of Basalt, either actually connected together by thinner parts in the lower grounds towards the dip of the strata of the district, or are detached hummocks, beyond the continuous edge of such a stratum, subject to the anomalies common (in kind at least) of all other strata: and to assure him, that I have never yet met with any single instance to the contrary: and with regard to Dykes I beg to ask,

8th. Have the Wallbottle and Coleyhill Dykes *wedged out*, as practical Men sometimes say, or have their cheeks met, and the fissures they occupied ceased, before reaching Montagu Main or East Denton Collieries?; or, does *a cross fault* or Dyke range between the places mentioned, at which these Dykes terminate, respectively, by joining into such cross fault?.

I perfectly join with Mr. Winch in concluding this head, that in treating not on Basalt only, but *on every* "peculiar species of rock, one should be contented to speak of it only *in situ*:" but this of course must not justify speaking or writing negatively, in the closet, before having examined *every part of the surface*, against *inferences from the facts* of other districts.

I beg respectfully to press again my 3d request, for more precise localities of Muscle-shell Ironstone: I cannot admit, that it is at all impossible to ascertain, to which of the particular measures, in the numerous sinking accounts with which Mr. W. has enriched the subject, the muscle-shells belong. The most illiterate Sinker or Sougher, whom I ever meet with, among some scores, on being shown a lump of the muscle-band, could almost instantly say, whether he had or had not sunk through a measure or bed thereof, in any particular Pit, of his recent sinking, and so of a Sough or Level, and if such Shells were found on his hillock, could always say, whence, precisely, they had been dug: and the comparison of various such independent accounts, might effectually guard against mistakes or imposition.

I shall continue anxious to read Mr. W's matured conclusions, as to the passing of Red Marl (or its Sandstone) under the Alum-shale, or otherwise, and hope nothing will occur to prevent the completion of his Yorkshire observations, and their speedy publication afterwards.

To

To conclude. *Old* red sandstone is a term become so truly ridiculous, from the perfectly contrary application of it, which equally confident Geognosts have made in England, that the sooner it is expunged from British phraseology, the better; nor can I think there is much sense, in substituting *new* in the place of "*vale of Eden* red sandstone," which latter no one could well mistake. It is not a little curious, that the site of this unconformable rock around Carlisle, is said, in the recently published 2d Edition of A Course of Lectures on Geology, pretending to give a copy and account of Smith's Map, to be occupied *by Graywakke!*: and similar assertions, are even more expressly made, regarding the vicinity of Liverpool, northward!—When, alas!, will it become the general practice in Geology, to *read and observe* first, and *write or Lecture* afterwards!.

I am,

Your obedient servant,

A Constant Reader.

P. S. The occurrence of Magnesian Limestone beds, in the vale of Eden strata, was very distinctly inferred, in p. 173 of your xlvth volume.

LXIV. *On the Length of the Seconds Pendulum,—on the reciprocal Action of Pendulums,—and on the Swiftness of Sound in different Substances,—by M.* Laplace:—*being the Substance of several Papers successively read to the French Academy of Sciences on the 28th of October, 25th of November, and 23d of December* 1816.

Length of the Seconds Pendulum.

The variation of gravity is the most proper phænomenon to enlighten us concerning the constitution of the earth. The causes upon which it depends are not limited to the terrestrial surface, but extend to the deepest beds or strata; so that an inconsiderable irregularity in a stratum situated a thousand leagues from the surface has a sensible effect on the length of the seconds pendulum. It may be concluded that the deeper this irregularity is, the more extended will be its effect on the earth: so that we may thus estimate that depth by the degree of corresponding irregularity in the length of the pendulum. It is very important, therefore, to give to the observations of this length such a precision as may assure us, that the anomalies observed are not owing to error. Already a great number of experiments on this subject have been made in both hemispheres; and although they leave much to desire, yet their regular progress in conformity

conformity with the theory of gravity, evidently indicates in the terrestrial strata, a harmony which they could only have acquired in a state of primitive fluidity, a state which heat only could have given to the whole earth.

The difficulties which present themselves in the measurement of the pendulum disappear, for the most part, when the same pendulum is transported to different points of the terrestrial surface. It may be said, indeed, that we thus obtain only the relative lengths of the seconds pendulum in these different places; but it is sufficient, in order to determine the absolute lengths, to measure its length with care in one of these places.

Of all the measures of absolute length, that which we owe to Borda appears to me to be the most exact. The little difference which the result of twenty observations offers, leaves no doubt of the exactness of their mean result. By applying to them my formula of probability, I found that an error of a hundredth of a millimetre would be an extreme improbability, if we were certain that there existed no constant causes of error.

In examining with attention the ingenious apparatus of Borda, a circumstance may be noticed, the effect of which, although very slight, is not to be overlooked in so delicate an inquiry. The pendulum is suspended from a knife, the edge of which rests on a horizontal plane. It has been supposed, in calculation, that the edge of this knife around which the instrument oscillates, is of infinite fineness; but on examining it with a microscope it presents the form of a demi-cylinder, the radius of which exceeds a hundredth of a millimetre. One might at first be led to believe that it would be necessary to add this radius to the length of the pendulum; but on reflection it may easily be perceived that this addition would be erroneous. The oscillation takes place every instant around the point of contact of the cylinder with the plane, and this point varies incessantly; it is only necessary therefore to make a calculation of the force which the pendulum experiences from the action of the weight, and from the friction of the knife upon the plane, in order to know the correction due to the radius of the cylinder or edge of the knife. In making this calculation, on the supposition that the knife does not slide upon the plane, I come to this singular result; that in place of adding the radius of the cylinder to the length of the pendulum, it is necessary to subtract it. This correction is less sensible, on the length of the pendulum, the longer the oscillations of the pendulum are. In the experiments of Borda it is reduced to a fourth of the radius of the cylinder, while it exceeds that radius in those of Messrs. Bouvard, Biot, and Mathieu, who for this reason must have found, and did actually find, a length of the seconds pendulum two hundredths of a millimetre greater

than

than that of Borda. It is remarkable that in applying the preceding correction to the results of two measurements, their difference should be reduced to less than a half a hundredth of a millimetre; a fact which proves at once the accuracy of the experiments made, and precision of the apparatus invented, by Borda, a precision which it would be very difficult to surpass.

If the edge of the knife were to slip upon the plane which supports it, the correction would depend on the law of resistance of friction, and it would become almost impossible to determine it. It is of use, therefore, to allow a few slight asperities to subsist upon the plane to prevent the knife from slipping. It is better also not to suffer the oscillation to be so strong as that the part of the edge of the knife that is in contact with the plane can overcome the friction which it experiences.

On the reciprocal Action of Pendulums.

The previous remarks on the measure of the seconds pendulum by Borda, have led me to examine particularly the different circumstances which may have an influence on this class of experiments, and the precautions to be taken to ensure the utmost exactitude of result—precautions which ought to be extreme, when it is sought to determine the length of the pendulum to a hundredth nearly of a millimetre. The most important of these consists in fixing the instrument in as solid a manner as possible, by attaching it to some very massive body, such as a thick wall, the particles of which are not themselves susceptible of extended vibrations. Daniel Bernoulli reported, in the Memoirs of Petersburg for the year 1777, an observation of Ferdinand Berthoud, who, having fixed an excellent astronomical pendulum very firmly, which before was rather loosely fixed, found that by this change alone it lost five minutes in one day—a fact which Bernoulli explains in a very ingenious and sound manner. Several time-pieces fixed upon the same bar impress it with a slight motion, which causes their particles to vibrate, and by the co-operation of these causes the time-pieces act one upon the other, and reciprocally modify their oscillations. Huygens in his work *De Horologio Oscillatorio* reports, that having thus fixed two time-pieces which went exactly alike, he saw them with surprise oscillate quite differently; the oscillations of the one always commencing at the same instant when the oscillations of the other terminated. It is still more remarkable that this should be the case even when there exists a slight difference in the going of two time-pieces placed apart from each other. Ellicot has made upon this subject some curious experiments, which he has recorded in the Philosophical Transactions of the year 1741; and M. Breguet has obtained similar results from

two

two chronometers placed very near each other. Many very curious effects of these vibrations have been observed by philosophers; and among these may be particularly distinguished the phænomena observed by M. Chladni upon sonorous plates and rods; from which this learned physician has deduced an ingenious method of determining the swiftness of sound in different solid bodies. The preceding researches have led me to the following theorem, to determine that swiftness in solid, in liquid, and in aëriform substances.

I assume that it has been determined by experiment, what degree of elongation a solid body placed horizontally, and fixed at one of its extremities, receives from the action of a weight equal to its own placed on its other extremity. If the substance is fluid, I suppose that it has been determined what is the abbreviation of a horizontal column of this fluid of the length of a metre and compressed by a weight equal to its own. This settled: if we divide by this degree of elongation or abbreviation double the weight which will depress the bodies in a second sexagesimal, the square root of this quotient will be the number of metres which sound traverses in that substance during the same interval.

Thus Borda having observed that a brass rod of eleven feet and a half in length, and 37 ounces in weight, was lengthened by the action of a weight of twenty-four pounds five parts and three-fourths, each part being a hundred-thousandth of a fathom; it follows that a rod of one metre will be lengthened $0^{m},000,000,77379$ by the action of a weight equal to its own. Dividing by that fraction the space $9^{m}\cdot 8088$, the double of what the weight draws at Paris, in the first second, the square root $3560\cdot 4$ of the quotient will be the number of metres which sound traverses during one second in brass.

It is known that the swiftness of sound in the air is increased about one-sixth by the heat which develops the approximation of the vibrating particles. The same cause must without doubt alter that swiftness in all bodies, but it is difficult to determine the exact influence of it. We may, however, approach near to such a determination, by comparing the swiftness deduced from the preceding theorem, with that resulting from the method of M. Chladni; for that method founded upon the sound produced by the longitudinal vibrations of sonorous rods, giving the real velocity of sound, the excess of that velocity above the preceding will be the effect of the temperature alternately elevated and reduced in the vibrations. The swiftness of sound, according to the experiments of Chladni upon a brass rod, is $3596^{mc}\cdot 58$ per second; which only surpasses the preceding in about a hundredth part. The influence of the cause alluded to

is then much less here than in air; but the slight errors of experiments leave this subject still in some incertitude.

In order to apply this result to fluids, I shall take water as the example. According to the experiments of Canton, recorded in the fifty-second and fifty-fourth volumes of the Philosophical Transactions, when the heat of the barometer is $0^{m},76$, the centigrade thermometer marking 10°, the pressure of the atmosphere diminishes the volume of water 42·5 millionths: the linear diminution is three times less. Thus a column of water of a metre in length is diminished 14·5 millionths by a pressure equal to that of a vertical column of the same fluid of the height of $10^{me},325$. The reduction of the first column compressed by a weight equal to its own is then $0^{me},000001,4044$. By dividing $9^{me},8088$ by that fraction 2642,8, the square root of the quotient will be the number of metres which sound traverses in water during one second;—its swiftness in this fluid is then nearly eight times greater than in air.

The experiments of Canton upon sea-water, the specific weight of which is 1,028, give 37,5 millionths as the diminution of its volume by the pressure of the atmosphere; whence it follows that sound must traverse salt-water $2807^{m},4$ in a second. These two proportions taken relatively to a temperature of ten degrees vary very sensibly with it.

Experiments to determine the velocity of sound in different substances, such as have been made in the preceding instances, appear to me well worthy of engaging the attention of philosophers.

On the Velocities of Sound in different Bodies.

Newton has given in the second book of his Mathematical Principles of Natural Philosophy, a theorem of the velocity of sound, which is one of the most remarkable traits of his genius. The velocity deduced from this theorem is about a sixth less than that which results from experiments made with great care in 1738 by the members of this Academy (French Academy). Newton, who had already discerned that difference from the experiments made in his time, has endeavoured to explain it; but the modern discoveries on the nature of atmospheric air have destroyed that explanation, as well as every other which geometricians have subsequently proposed. These discoveries fortunately present us with a phænomenon which appears to be the true cause of the excess of swiftness observed in sound, above the degree of swiftness calculated, and which the greater part of philosophical geometricians have since adopted. This phænomenon consists in the heat which the air develops by its compression. When its temperature is raised, its pressure remaining the

the same, a part only of the caloric which it receives is employed to produce that effect. The other part which remains latent serves to dilate its volume; and it is this which develops itself when the air thus dilated is reduced by compression to its original volume. The heat disengaged by the approximation of two neighbouring particles of an aërial vibrating fibre, increases their temperature, and spreads itself gradually over the air and surrounding bodies: but this diffusion and irradiation taking place with an extreme slowness relatively to the swiftness of the vibrations, it may be reasonably supposed that during the period of a vibration the quantity of heat remains the same between two adjoining particles. Thus these particles in approximating repel each other more at first, because, their temperature being supposed constant, their mutual repulsion augments in an inverse ratio to their distance; and afterwards, because the latent caloric which develops itself raises their temperature. Newton had only regarded the first of these two causes of repulsion; but it is evident that the second cause ought to increase the swiftness of sound, since it augments the elasticity of the air. By taking this into the calculation, I arrive at the following theorem:

"The real swiftness of sound is equalized to the product of swiftness which the Newtonian formula gives by the square root of the affinity of the specific heat of air submitted to the constant pressure of the atmosphere and to different temperatures, to its specific heat when its volume remains constant."

If we suppose, with many philosophers, that the heat contained in a mass of air submitted to a constant pressure and to different temperatures is proportional to its volume (which would be deviating a little from the truth), the preceding square root will become that of the affinity of the difference of two pressures to the difference of the quantities of heat which two equal volumes of atmospheric air submitted respectively to these pressures develops, in passing from a given temperature to an inferior temperature; the smallest of these quantities of heat and the smallest of these pressures being taken for unities.

Being desirous of comparing this theorem with experience, I have fortunately found the data of the observation which it supposes among the numerous results of the interesting work of M.M. La Roche and Berard upon the specific heat of gas. These able philosophers have calculated the quantities of heat which two equal bodies of atmospheric air emit by a reduction in temperature of about eighty degrees;—the one compressed by the weight of the atmosphere, the other by the same weight augmented $\frac{36}{100}$. They have found that the heat disengaged, relative to the greatest pressure, was 1·24; the heat relative to the smallest pressure being unity. It is necessary then according to

the preceding theorem, in order to obtain the real swiftness of sound, to multiply the swiftness deduced from the formula of Newton by the square root of the affinity of $\frac{36}{100}$ to $\frac{24}{100}$, or by the root of $\frac{3}{2}$. To the temperature of six degrees, this formula gives 282·42 for the space which sound ought to traverse in a second: multiplying that by $\sqrt{\frac{3}{2}}$, that space ought to be equal to 345·35. The observation of the French academicians makes it only 337·18. The difference of these two results may be owing to the inaccuracy of the experiments; but the smallness of that difference establishes in an incontestable manner, that the excess of the swiftness observed above the swiftness calculated by the Newtonian formula, is owing to the latent heat which the compression of the air develops.

It results from what has been stated that, supposing the pressure constant, if we augment a given volume of air by elevating its temperature, and afterwards reduce it by compression to its original volume, it will disengage by that compression a third of the heat employed. It is to be desired that philosophers would determine by direct experiments the affinity of specific heats of the air to constant pressure, and of the air to a constant volume:—this affinity we have found equal to 1·5. The swiftness of sound observed by the French academicians gives 1·4254 for this affinity; and perhaps, considering the difficulty of direct experiments, this swiftness is the most precise means of obtaining it.

I have in the second part of these observations reckoned the swiftness of sound in rain-water and in sea-water equal to 2642·8 and 2807·4 per second, by dividing the experiments of Canton upon the compression of these liquids, and having only regard to the linear diminution of the dimensions of the compressed volume. I have since found that it is necessary to consider the *total* diminution of this volume, and that the preceding numbers ought thus to be divided by $\sqrt{3}$, which reduces them to 1525·8 and 1620·9; so that the swiftness of sound in fresh-water is four degrees and a half greater than in air.

LXV. *On the Principles of Beauty in Colouring.* By Mr. T. Tredgold*.

If the term *beautiful* be restricted to those objects only which give pleasure from the pleasing associations they recall to the beholder, then the investigation of any other causes, which may contribute towards rendering objects beautiful, would be an useless labour; for this limited definition of beauty precludes all inquiry, and is as unsatisfactory as it is unphilosophical. On

* Communicated by the Author.

the

the contrary, if beauty be a term for that quality of objects which renders them capable of exciting an emotion of pleasure, the cause of that emotion becomes an interesting object of inquiry.

The emotion excited by the presence of a beautiful object may either arise from the peculiar constitution of our organs of sensation, or from the pleasing associations it recalls to the mind: but, in general, the emotion may be considered as the combined effect of both these causes.

In inquiries of this kind we cannot expect to show the correctness of our reasoning by a detail of experiments; but when it leads to principles which coincide with or approach to those followed by artists who have cultivated their natural powers of perceiving the beautiful, we may justly conclude that it is correct. Those who have never studied geometry would be very incompetent judges of the beauty of a geometrical demonstration;—and is it less inconsistent to expect those who have never examined the effect of different combinations of colours, to be capable of deciding on the comparative beauty of an arrangement of colours? Colours, in themselves, perhaps, cannot be considered as beautiful when divested of the associations which accompany them; but a succession or combination of colours may be beautiful, and that not from any physical or intrinsic quality in the colours, but from the peculiar structure of the organ, and the manner in which it transmits the impressions to the mind. Instances of such combinations may be seen in the rainbow, the prismatic spectrum, coloured rings, fringes*, minerals †, flowers, &c.

A late writer, who affects to deny that the beauty of colours depends on particular successions or combinations, admits that in the case of sounds "*there is* such an organical delight; and that it constitutes a larger share of the *beauty* of sounds, than tints and shadows do of the *beauty* of visible objects ‡." And in another publication, he says, he does not "mean to dispute, that there are such things as melody and harmony, and that most men are offended or gratified by the violation or observance of those laws upon which they depend. This, however, it should be observed, is a faculty quite *unique*, and unlike any thing else in our constitution; by no means universal, as the sense of beauty is, even in cultivated societies, and apparently withheld from whole communities of quick-eared savages and barbarians §."

Now in this there is something extremely inconsistent: for though the mere musical arrangement of sounds may not excite

* See Brewster's papers on those subjects, Phil. Trans.

† See Sowerby's work on Colours. ‡ Edin. Review, No. xxxv. p. 39.

§ Supp. Ency. Brit. art. *Beauty*, 1816.

the emotion of beauty; yet it will scarcely be said that sounds without melody or harmony would excite such emotions;—and therefore the beauty of musical compositions must in some measure depend on the succession and combination of agreeable sounds.

It will be obvious to the reader that I confine the term *beautiful,* to those emotions which are suggested to the mind by means that are agreeable to the organs of sensation. This forms the distinction between the beautiful and sublime; as it does not appear necessary that sounds should be agreeable to produce the emotion of sublimity.

I am not aware that the ears of savages and barbarians are in any respect different from those of other men; and there are physical reasons from which we may conclude that they would be equally sensible of the melody of sound with their more civilized brethren, if their natural powers were not vitiated by the powerful influence of early habits. As sound is communicated by the vibration of sonorous bodies, when there is not a certain relation between the times of vibration of coexistent sounds, it is obvious that, though the emotion excited may not be absolutely painful, except to a musical ear, yet we feel dissatisfied with it,—habit will make us insensible of the disagreement, and habit will render the ear sensible of it to a painful degree. To what can we attribute the admiration which is excited by the skill and power of the composer, if that skill and power be not directed to the end of producing something agreeable? And will not the beauty of the composition be in proportion to his success in rendering it agreeable to the ear?

We know that the eye, when it has adapted itself to a particular degree of light, is painfully affected by a sudden change. The same thing happens, though not in a painful degree, when it has been directed for some time to one colour, and is suddenly directed towards an opposite colour. The disagreeable effect may be caused by the different-coloured rays being projected on the organ of sensation with different degrees of velocity. But the writer of the article Beauty (*Ency. Brit.*) is not a little sceptical on these subjects, and is much more inclined to ridicule than to investigate. He even supposes that if all the colours in Nature were disposed on a broad pannel, according to the nicest rules of harmony, it would be doubtful if any "grown creature" would call the display beautiful*. It would have been better if, before repeating this assertion†, he had procured the assistance of some friendly artist, to transpose for him the colours of the prismatic spectrum. This simple ex-

* Edin. Review, No. xxxv. p. 37. 1811. † Supp. Ency. Brit. art. *Beauty*, p. 193. 1816.

periment

periment would have furnished him with a practical refutation of his opinion.

If we suppose a piece of canvass to be covered with unmeaning masses of light and shade, richly and harmoniously coloured, and the light and shade distributed by the tasteful hand of a superior artist; there cannot be a doubt respecting the beauty of such a composition: and it is evident that its beauty proceeds from two causes; the one arising from association, the other from sensation. The pleasing effect produced on the eye would lead us to admire the skill and knowledge displayed by the artist; and the effect would not be diminished, at least in the eye of a trained judge, when he found that it was not an imitation of any particular object.

What is it that constitutes the beauty of minerals? There we have no artist's skill nor power to associate; neither is there any pleasing indication of spring or summer, as in flowers;—is it not to the pleasures of sensation alone that they owe their beauty? It is true, as Mr. Knight has justly observed, that "the scale of pleasing and displeasing impressions cannot be graduated according to any abstract general rule, but must be adapted to the different degrees of sensibility of different organs; which vary, not only constitutionally, but habitually; the eye, as well as the palate, being liable to be vitiated, and consequently to require such stimulants to give it pleasure, as give pain to those of more refined sensibility*."

The Russian, who has acquired a taste for train-oil, and the Dutchman, who paints his house with all the gaudy colours which he finds in his tulip bed†, no doubt, were born with tastes and faculties similar to those of other men.

The power of perceiving the beautiful, like every other natural power, may be improved and extended by education and a habitual attention to the beautiful in Nature; and, like our other faculties, it may be impaired or lost through neglect.

In the composition of colours, it is not necessary that they should be in perfect harmony. A judicious contrast frequently adds much to the beauty of colouring; and the *taste* of the artist cannot be displayed to a greater advantage than in the skilful management of this mode of heightening the *beauty* of his compositions.

Mr. Alison, speaking of form, says "that it ought to be the unceasing study of the artist, to disengage his mind from the accidental associations of his age, as well as the common preju-

* Analytical Inquiry into the Principles of Taste, p. 65.
† Article *Beauty*, Supp. Ency. Brit.

dices

dices of his art; to labour to distinguish his productions by that pure and permanent expression, which may be felt in every age; and to disdain to borrow a transitory fame by yielding to the temporary caprices of his time:" for "the fame of the artist must altogether depend upon the permanence of the expression which he can communicate to his work; and the only expression which is thus permanent, and which can awaken the admiration of every succeeding age, is that which arises from the nature of form itself, and which is founded upon the uniform constitution of Man and of Nature*." Such views should also be his guide in colouring: for fashions may vary, and the manners and habits of nations may change; but the principles of colouring will always be the same;—they are founded in the "constitution of Man and of Nature," and may, at any time, be elicited by the unbiassed powers of reason.

LXVI. *A further Account of the Exhibition and Harmonic Effects of the Rev. Mr.* LISTON'S *large* EUHARMONIC ORGAN *with Compound Stops. By* A CORRESPONDENT.

To Mr. Tilloch.

SIR, — AT the time I drew up in great haste, towards the end of last month, a few particulars, which you have done me a great favour in so promptly inserting at p. 213 of your last Number†, as to *Mr. Liston's* novel and grand ORGAN then exhibiting, I was unaware that Professors and Amateurs would so suddenly be deprived of the high gratification they were increasingly receiving, in flocking to try and hear it, by the necessity which has occurred, sooner than was expected, of taking to pieces and packing up this Instrument, to send it on board the Castlereagh, Capt. Younghusband, who is to convey it to Calcutta, with Mr. *John Alsager*, a very intelligent young Organist, who had been selected by the congregation of Presbyterians or Scotch Church at Calcutta, to superintend Messrs. Flight and Robson's operations in building this Organ, and Mr. Liston's in tuning it; in which latter, and essential operation, Mr. A. has perfected himself, as well as become expert in the use of the pedals, and who from the zeal and abilities he manifests, is likely to do great credit to the discernment of the parties in selecting him.

So here, contrary to all which might have been anticipated, from the rooted aversion still shown by the Scotch Congregations,

* Essays on the Nature and Principles of Taste, vol. ii. p. 200.

† In line 11 from bottom, for *sharps* read "flats."

not

not in retired villages only, but in the Metropolis and in Glasgow, *to the use of any Instrumental Music whatever* in their Kirks, we see the singular example, and laudable spirit displayed by the members of this same religious community, in a distant colony, in being the first to order an Euharmonic Organ of any sort!: and that on so grand a scale, with compound Stops. While by the English nation, who affect to consider an Organ, almost as a necessary appendage to every polite Church or Chapel, Mr. Liston and his friends have been left to bear the heavy charge of constructing three successive Organs, exhibiting them gratis, and issuing prospectuses, &c. without the public having given to the really scientific improvement Mr. L. has effected, in the most delightful of Arts, the least effective patronage whatever!; the sale of his "Essay on perfect Intonation," not having paid its expense of printing, as the writer understands, who in justice to himself begs to observe, that he is without the most distant motives in making these remarks, besides those of zeal in the promotion of the harmonic science, and to serve ingenious and deserving Individuals, who have promoted it.

Since the performances to which I alluded in my last communication, His Royal Highness the Duke of Sussex, the Earls of Buckingham and Kenton, Lord and Lady Darnley, and several other persons of distinction, the members of the Philharmonic Society, and on the whole not less than 500 musical persons attended the performances on this Organ: in which, among other pieces

Mr. Purkis played from the works of *Pergolesi*, "Gloria in excelsis:"—from those of *Winter*, "Vaghi Colli" in Proserpine, in the Key of A♭, instead of A, as set: and one of his Airs in F minor:—from *Haydn*, "On thee, each living Soul awaits:" and, "The Heavens are telling," in B 5 sharps, instead of C. —From *Handel*, several pieces from the Dettingen "Te Deum;" "Surely he hath borne:" "And with his Stripes:" "All we like Sheep;" and, "For unto us a Child."—And from *Mozart*, the Overture to Don Giovanni.

Mr. Potter also gave his assistance in ably exhibiting the Euharmonic Organ, viz. from the Works of *Haydn*, a movement from The Passion.—from *Sebastian Bach*, "Tantum Ergo."—from *Mozart*, "Qui Sedes:" "Quoniam Tu Solus:" and, "Et resurrexit."—from *Joachim Dos Santos*, "Cum Sancto Spiritu;" and "Crucifixus etiam."—from *Handel*, "Lift up your Heads." —from D. *Perez*, "Et incarnatus est."

A Table

A Table of Two Octaves of the Scale of The Rev. HENRY LISTON'S *large Euharmonic Organ, exhibited in March* 1817, *at Messrs.* FLIGHT *and* ROBSON'S *Room, in St. Martin's Lane, London.*

Showing, how the 79 Notes are *Literally* distinguished; their *Values* in Mr. FAREY'S new Musical Logarithms or *Artificial Commas**, and the *Numeral* designations of the 78 Intervals they form, above C.

Notes.	Artificial Commas.	Intervals.	Notes.	Artificial Commas.	Intervals.
c	612	VIII, or Octave	c	1224	XV or double Oct.
B′*	602	Extreme *VII′	b′*	1214	Extreme *XIV′
c`	601*	VIII`	c`	1213	XV`
B*	591	Extr. *VII	b*	1203	Extr. *XIV
c′b	576	8′	c′b	1188	15′
B′	566*	VII′	b′	1178	XIV′
cb	565	8, or Extr. bVIII	cb	1177	15, or Extr. bXV
B	555	VII	b	1167	XIV
B`	544	VII`	b`	1156	XIV`
B′b	519	7′	b′b	1131	14′
Bb	508	7, or Extr. bVII	bb	1120	14, or Extr. bXIV
A*	498	Extr. *VI	a*	1110	Extr. *XIII
A′	462	VI′	a′	1074	XIII′
A	451	VI	a	1063	XIII
Ab	415	6, or Extr. bVI	ab	1027	13, or Extr. bXIII
G′*	405	Extr. *V′	g′*	1017	Extr. *XII′
G*	394	Extr. *V	g*	1006	Extr. *XII
G′	369	V′	g′	981	XII′
G	358	V	g	970	XII
G`	347	V`	g`	959	XII`
G′b	322	5′	g′b	934	12′
Gb	311	5, or Extr. bV	gb	923	12, or Extr. bXII
F*	301	IV, or Extr. *4	f*	913	XI, or Extr. *11
F`*	290	IV`	f`*	902	XI`
F′	265	4′	f′	877	11′
F	254	4, or Extr. bIV	f	866	11, or Extr. bXI
E*	244	Extr. *III	e*	856	Extr. *X
E`*	233	Extr. *III`	e`*	845	Extr. *X`
E′	208	III′	e′	820	X′
E	197	III	e	809	X
Eb	161	3, or Extr. bIII	eb	773	10, or Extr. bX
E`b	150	3`	e`b	762	10`
D*	140	Extr. *II	d*	752	Extr. *IX
D	104	II, or T	d	716	IX
D`	93	II`, or t	d`	705	IX`
Db	57	2, or Ex. bII, or H	db	669	9, or Extr. bIX
C*	47	I, or Extr. *1	c*	659	Extr. *VIII
C`*	36	I`	c`*	648	Extr. *VIII`
C′	11	1′, or Comma	c′	623	VIII′
C	0	1, or Unison	c	612	VIII, or Octave

* An *Artificial Comma* (or Schisma) is the *smallest Interval* which can occur in a euharmonic or perfect scale of Music, however extended; is marked *, and is found here between cb and B′, and between c` and B′*.

Mr. Alsager exhibited various new and unexpected results, of chords *truly tuned* on this Instrument, such as the marked and pleasing character of the IX, very closely associating it with the less grateful of the *Concords*; and he said that this interval could be almost as certainly adjusted by the ear at once in tuning, as one of the Concords: the same with regard to the *VI: and the b10,(726); the perfectly smooth and agreeable effect of the discords XIV and XVI, when heard in the common Chord on the Twelfth and Diapason Stops, and others, which it might be tedious to mention.

It having been hinted by some of the Profession, to whom Messrs. Flight and Robson had handed the printed Paper copied into your last, that a more detailed Table of two Octaves of the Scale of the Instrument would be desirable; these gentlemen in their laudable zeal to explain the principles of the Instrument most fully, have since printed and circulated such a Table, which I cannot doubt but your Readers will approve of seeing preserved in your Magazine. See the preceding page.

I hope the zeal at present beginning to be manifested for encouraging Euharmonic Organs, will be productive of their spread amongst us, and I am

Your obedient servant,

April 6, 1817. Philo-Musicus.

LXVII. *Observations on Equation of Payments; tending to prove that the generally-received Rule of Malcolm is not correct. By Mr.* J. B. Benwell.

To Mr. Tilloch.

Sir, — In the cessation from other engagements, I send you the following observations for insertion in the Philosophical Magazine.

It has been asserted, and with evident reason too, by mathematical writers of the present day, that no rule in arithmetic has been subject to more dispute than the rule for equating of payments. But as in the case of the binomial theorem mathematicians have long been seeking a general demonstration from principles too refined and foreign to its object, so in this rule called equation of payments, a principle has been assumed which (although accurate in itself) will not apply as the basis of a legitimate datum, excepting by virtue of an express convention previously and purposely made.

But it would really seem that the controversy had been effectually decided by the unanimous adoption of Malcolm's rule, as simplified since his time, and which may be found in the few

treatises

treaties appertaining to scientific, that we at present possess on the subject.—That the rule, however, is susceptible of further elucidation, I shall endeavour to exemplify in the sequel of consideration. With a view of preparing to illustrate my position, I shall abstract both the rules in their analytical form (which I shall do from Bonnycastle's Arithmetic), and subsequently their practical enunciation :

$$x = \frac{1}{2} \cdot \left(t + \frac{a+b}{ar}\right) - \overline{\left(\frac{a.r.t + a + b}{2ar}\right)^2 - \frac{bt}{ar}}^{\,\frac{1}{2}}$$

Here a. the first debt. b. the second. $t =$ time between the payments. r. the rate; and x. the equated time from the first payment.

$$x = \frac{\mathrm{DT} + dt}{\mathrm{D} + d}.$$

Here $d =$ the first debt, and its term t. $\mathrm{D} =$ the second, and its term T. x. the equated time.

The first expression is Malcolm's rule, and the second is that which I may properly term the old rule, as adopted by Cocker, and nearly all the authors on practical arithmetic who have followed in succession after him. It depends on the supposition, "that the sum of the interests of the debts payable before the equated time from their terms to that time, should equal the sum of the interests of the debts due after the equated time from that time to their terms." But which also includes this extension; viz. "that the interest of the sum of the debts from the equated time to the end of the final term is equal to the sum of the interests of the debts from their several terms to the end of the final term *."

And this I conceive to be the true intent and principle of the rule; namely, an equation of the party's separate right and interest in the debts, so that neither shall sustain loss;—or where is the utility of determining an equated time at all? and which also I take for granted was the primary object contemplated by the founders of it. But this object can never be accomplished in Malcolm's rule; although, from the plausibility of making a proper allowance of discount, those who argue in favour of the rule maintain that it is: but it is an absolute fact, that one party will be injured by it, and that party is always the debtor.

The main reason why Malcolm's rule fails in giving the true

* For let there be two payments: a. the first, and b. the second; $r.t.x.$, denoting the same quantities as before; then $(a.r.t)$ will be the entire interest arising from the debts at the end of the last term; and to equate this for the time $(t-x)$ we should have $(a.r.t) = (a.r.t + b.t.r - a.r.x + b.r.x)$, and by reduction $((a+b).\ rx) = (b.r.t)$; or, $(b.r.t - (a.r.x + brx)) = 0$; from which the condition is manifest.

equated

equated time is owing to the very assumption made in it, which directs us to find the "time when the interest of the debt kept after it is due from its term to the equated time shall be equal to the discount of the debt, for the time it is paid before due." But here this time (which is so restricted as to agree in such particular respect) I contend is arbitrarily assigned by our own express convention; so likewise is the principle; neither of which arises out of the known and obvious conditions involved as well as aimed at in both the rules (if they did, then the rule would be strictly correct), viz. "that the gain and loss being equal, neither party can sustain injury." For as well might we change the principle above, assuming as another the converse of it, and then investigate a rule for such case accordingly. But as to the truth of what is contained in the foregoing inferences, I shall further proceed in an attempt to establish by the solution of a practical example agreeably to both rules: and so as to abridge the resulting process, I shall introduce the case of two payments only; since, according to the principle on which the common rule depends, if it can be proved to be the true rule for two payments, it must necessarily be so for n. payments. A owes B 205*l.* 100*l.* to be paid one year and 105*l.* three years hence: required the equated time to pay the whole. First by the common rule.

$$\begin{array}{l} 100 \times 1 = 100 \\ 105 \times 3 = 315 \\ \overline{205} \quad 205 \overline{\div 415} = 2\tfrac{1}{41} \text{ years the equated time.} \end{array}$$

$$205 + (100. \times 05 \times 2) = 215 \div (100. \times 05. \times 2) = 21{\cdot}5$$

$$(21{\cdot}5^2 - 42)^{\frac{1}{2}} = 420{\cdot}25^{\frac{1}{2}} = 20{\cdot}5$$

$21{\cdot}5 - 20{\cdot}5 = 1$ year from the first payment, or two years the equated time.

Now according to the first solution neither party will be the loser: it is therefore obvious this circumstance cannot arise from the latter. Hence resuming the question under a different form of calculation by allowing the true proportion of discount only; let us observe how the result accords with that just obtained.

When the time for the first payment arrives, let it be supposed that A chooses to make them both: then if a. is the first debt, and b. the second, $(a + \frac{b}{1+rt}$ will be the sum to be paid down, or $\frac{b. \times r. \times t}{1+rt}$ the discount to be allowed. In this view of the question, then, it is plain that B will not sustain a loss; for this is a circumstance on his part we ought not to lose sight of any more than that of A. Now whether A adjusts the payment in this manner, or retains the sum b. until the period it is due, is equally indifferent; as it is certain when that arrives he will be in possession

session of the interest of the sum $b.$ for the time $t.$ between the payments. But here let it be proposed to find an equated time, so as to make the whole at one payment;—let that time, be what it may, be denoted by z. And I shall endeavour to seek it without regard to any rule whatever: and from what has gone before I think it must be evident this time will be the true time, if it be such that our object is fulfilled.—"By neither party being the loser," it has appeared as an inference from the above equation, that at the end of the second or last term, A will have acquired the whole interest $(b. \times r. \times t)$, and for any intermediate time an interest will be derived proportional thereto. Now $\left(\frac{b.r.x}{1+rt}\right)$ is the portion of interest arising from $\frac{b}{1+rt}$ for the interval between the first payment and time z; but by hypothesis the sum $a.$ is also to be retained for the time $x.$; and in order that the equation shall be true for $x.$ in particular, we must have $\left(arx+\frac{b.r.x}{1+rt}\right)=\left(\frac{b.r.t}{1+r.(t-x)}\right)$: but it may be seen in this instance as dependant on the value of x that $\frac{b.r.t\times r.x}{1+rt}=\frac{b.r.t\times r.(t-x)}{1+r.(t-x)}$.*Therefore adding these equal quantities on both sides we get $(a.r.x+b.r.x)=b.r.t$, as before. Whence $x=\left(\frac{b.r.t}{(a+b).r}\right)$ the equated time from the first payment: and from which expression, using the same quantities as in our present example, we obtain

$$z-1=\left(\frac{105\cdot\times\cdot05\times2}{(105+100)05}\right)=1\tfrac{1}{41}\text{ or }2\tfrac{1}{41}\text{ years;}$$ the whole equated time: Corresponding exactly with the time as found by the common rule; and as a proof of the accuracy of which we shall find upon trial that the quantity $\left(a+\frac{b}{1+tr}\right)\times r.x$ will, if exployed at interest for the further time $t-x.$ exactly amount to or equal $(b.r.t)$; but if we take the time as given by Malcolm's rule, the former quantity with its accretion for the given excess will evidently be less than the latter. Also we have $(a.r.x)=b.r.(t-x)$ the equation from which the principle of the rule is directly derived.

By pursuing a similar mode of inquiry, whatever the number of payments may be, it will lead precisely to the same results. That there is an inconsistency in the conditions of Malcolm's rule must be apparent without any previous examination; since

* The coincidence in this respect is extremely apposite, and besides it supersedes the necessity of a tedious and operose process.

it

it will not apply in those cases where more than two payments are concerned; by first equating for two, then a third, and so on, a difficulty which the advocates for it have never attempted satisfactorily to explain; yet it is said the equated time may be found for any number of payments when the question is propounded in numbers; but that it would be difficult to give a general theorem for such cases: analytically, indeed, the thing is I believe hardly possible to be effected.

From the preceding discussion I feel persuaded it will appear that the old rule is the true, and Malcolm's the false one; and I have been the more solicitous to point out the distinction between the rules, as well as to explain on what head I conceive the ambiguity in Malcolm's to rest, because several eminent writers who rank high on the score of science have indiscriminately one after another adopted it in their works, under the impression of its being the only true rule: but if this mode of reasoning be just, they must have rendered, and so will all succeeding writers (who give the rule admission into their works) at least render them erroneous in this respect. But if otherwise it can be shown that what is herein advanced is not consonant to the truth, as I am always open to the conviction of errors, so I shall be thankful, on my part, for the candid correction of them.

I am, sir,

Your very obedient humble servant,

Jas. Benj. Benwell.

Haberdashers Place, Hoxton,
April 12, 1817.

LXVIII. *Answer to Objections against Mr.* Horn's *Theory of Vision.*

To Mr. Tilloch.

Sir, — I perceive by your last Number, that the opponents to my theory have increased:—still I am neither appalled by their number nor arguments. I am disposed to draw a broad line of distinction between Mr. Pater and your correspondent J. Q. R. The latter gentleman's argument assumes something like a tangible shape; though I have to complain of his *ex parte* decisions. As my principles are already before the public, it is rather preposterous to ask, "What foundation has Mr. Horn for his theory?"

Your correspondent L.S. has partly avoided this inconsistency. Being in possession of my pamphlet, he has also corrected some of the errors into which Mr. P. and J. Q. R. have fallen. But while he does this with an apparent regard for justice, in order to render my optics ridiculous, he grossly misrepresents the description I have given of a phænomenon, which he found could not

not be accounted for on any legitimate principles but by admitting the truth of my theory.

Now, sir, lest the imperfect view L. S. has given of the principles of my theory, and the confidence with which his "single objection" is urged, should prejudice the readers of your valuable Magazine, I mean to show that this redoubtable objection is of no avail; and that the claims of the *retina* to the chief function of vision are not only very doubtful, but that those of the optic nerve are more consistent with the laws of optics, and better supported by phænomena.

J. Q. R. does not seem to be aware of the difficulties that have presented themselves to men eminent for science, in their investigations into the seat of vision, or he scarcely would have spoken so confidently respecting the *structure and use of the retina.* I have before noticed (Philosophical Magazine, vol. xlviii. p. 118) the celebrated controversy between Marriotte and Pecquet respecting the retina and choroid membrane. De la Hire, though he took part with Pecquet, arguing in favour of the retina, supposes that our defect of vision, where the optic nerve enters, is produced by the want of the choroides: accordingly, he conceives that the retina does not receive the impressions immediately from the rays, but that they penetrate it, are stopped by the opacity of the choroides, by which this membrane is agitated, and these agitations being communicated to the retina, are transmitted to the nerve; and thus vision is effected.

When Dr. Priestley was composing his History of Vision, Mr. Michell suggested a number of additional arguments in favour of the choroides as the immediate organ of vision, which had escaped its former advocates. These, though very ingenious, I shall not enumerate. "I must own," says Dr. Priestley, "that after having retained my prejudice in favour of the retina, notwithstanding all that was advanced by Marriotte and the advocates for his opinion among his countrymen, the arguments of Mr. Michell, in favour of the choroides, have more weight with me than I was at first either able to perceive, or willing to acknowledge, in those of the French philosophers." However, the learned author, towards the conclusion of his History, in an additional section relating to the seat of vision, gives up his opinion of the choroides as the proper organ, and inclines to the hypothesis of De la Hire.

But these are not the only remarkable features in the physiology of the eye. Even those who contend for the retina, as an expansion of the optic nerve and the principal organ of vision, express themselves either doubtfully, or are at open variance with each other, respecting the structure and requisite sensibility of this membrane.

Haller,

Haller, rejecting the hypothesis that goes to establish the choroides as the proper instrument of vision, speaks of a certain fibrous membrane *in* the retina, distinct from its pulpy substance, and supposes that on these fibres the images of the objects are painted.

"We have reason," says Dr. Reid, "to believe that the rays of light make some impression upon the retina; but we are not conscious of this impression; nor have anatomists or philosophers been able to discover the nature and effects of it;—whether it produces a vibration in the nerve, or motion of some subtile fluid in the nerve, or something different from either, to which we cannot give a name."

Dr. Priestley observes, among *desiderata* at the end of his History of Vision, that "something may possibly be added more decisive for or against the retina being the place where the pencils of rays terminate,—or, in other words, being the *seat of vision*,—than has as yet been advanced by the advocates for either of the opinions."

Besides the suspicious circumstances already mentioned in the character of the retina, there is another point which is not a little embarrassing to the advocates for its being the seat of vision. Although this membrane is expanded over the whole concavity of the eye as far as the *ligamentum ciliare*, they acknowledge that it is not all equally sensible. The ingenious Dr. Porterfield, in his treatise on the eye, observes, that this is evident;—if, for instance, we are disposed to view the first letter of any long word accurately, the other letters, especially those towards the end, will not appear clear and distinct. The cause of this, he says, does not so much arise from the pencils of rays coming from those letters falling obliquely upon the retina, but chiefly because of a certain degree of hardness, callosity, or *insensibility*, in all parts of the retina, excepting towards the axis of the eye, directly opposite the pupil.

But this opinion is contradicted in a more recent hypothesis advanced by Mr. Walker in his Archives of Science. This author supposes that the images of the external objects are first formed at the posterior portion of the retina, and by reflection from thence the impressions are made upon its anterior part. "Consistently," he says, "with the theory just delivered, I should conclude, that we have from this circumstance a decided proof that the *posterior* part of the retina is utterly insensible; since at the entrance of the nerve, where it exists in the greatest quantity, it can be demonstrated to be so; and that vision is wanting at this spot precisely, because where the nerve enters there is no choroides to reflect the rays to the *sensible* anterior portion."

 "It

"It is evident," says Dr. Reid, "that the pictures upon the retina are by the laws of Nature a mean of vision; but in what way they accomplish their end we are totally ignorant."

From the different hypotheses respecting the seat of vision, and contradictory opinions as to the sensibility of the retina, the immediate inference that every unprejudiced inquirer must draw, is, that their respective authors were alike uncertain as to the structure and use of this membrane:—each found himself perfectly at liberty to accommodate its character to his own hypothesis.

But whatever the difficulties may be respecting its texture, the nervous nature and origin of the retina are supposed demonstrable from the constitution of the optic nerve. It is allowed by anatomists, that the nerve possesses two tunics that envelop its medullary substance;—the exterior, derived from the *dura mater*, forming by its expansion the sclerotic coat of the eye; and the interior, which is a continuation of the *pia mater*, expands itself on entering the eye, and forms the choroides. The retina or innermost coat of the eye is therefore supposed to be a propagation of the nervous substance; the entire trunk of the optic nerve being thus naturally expanded into the principal coats that compose the globe of the eye.

Now, however natural this arrangement of parts may appear, yet I conceive the fact, as far as it relates to the retina, to be very questionable.

In the first place, the controversy so ably carried on, without clearly deciding the question, whether the retina or the choroides be the principal organ of vision, leaves the character of the former in suspense. To say nothing of those inadequate hypotheses which Pecquet was forced to invent, in order to account for its insensibility upon the base of the nerve, some of its advocates strenuously contend, that the retina is every where insensible, except at the axis of the eye. Others, with equal confidence, affirm that it is totally *insensible* at the *axis*, and confine its sensibility to the anterior portion. On the contrary, Marriotte and others, who plead for the choroides, assert that the retina is *insensible throughout its whole structure*. Le Cat, who held that the *pia mater*, and not the nerves themselves, is the proper instrument of sensation, supposes that the retina answers a purpose similar to the *scarfskin* covering the *papillæ pyramidales*, which are the immediate organ of feeling. The retina, he says, receives the impressions of the rays, moderates and prepares them for the proper organ, but is *itself insensible* to any impression from the rays of light.

In the second place, it appears highly unreasonable to suppose so large a mass, as the medullary substance of the optic nerve,

nerve, necessary for the production of a membrane so very thin and delicate as the retina appears to be in its texture. Some proportion certainly is discoverable between the sclerotic coat and the *dura mater*, and also between the *pia mater* and the choroid membrane;—but can any necessary correspondence be discovered between the volume of matter contained in the trunk of the optic nerve, and the small quantity requisite to form that thin lining for the choroides, called the retina?

In the last place, no useful purpose has ever yet been assigned, nor can possibly be conceived to arise from leaving a spot equal to the base of the nerve destitute of the choroides, and totally insensible to the rays of light. Besides, to impute either a total or partial insensibility to any disposition of a substance which is allowed by every physiologist to be the grand medium in the animal œconomy, through which impressions on the organs of sense are communicated to the common sensory, is, to say the best of it, an unaccountable anomaly in the nervous system.

I am, sir,
Your obedient servant,

Wycombe, April 7, 1817. ANDREW HORN.

LXIX. *On the Priority of the Transmission of Cow-pock Matter to America.* By T. J. PETTIGREW, *Esq.*

To Mr. Tilloch.

SIR, — IN the last Number of your Magazine, a correspondent under the signature of "Play-Fair" expresses his surprise to find it stated in my Memoirs of the Life and Writings of the late Dr. Lettsom, that the vaccine lymph was first sent across the Atlantic by Dr. L., and consigned to the care of his friend Dr. Waterhouse of Cambridge, Massachusetts, from whence it *spread* through the United States. This is said to be untrue; That "vaccine lymph had been previously sent by Dr. Geo. Pearson to Dr. Chichester, now a resident physician at Bath, but at that time in very extensive practice at Charleston in South Carolina." Reference is also given to the Philosophical Magazine, vol. xvi. p. 252, for the particulars of the vaccination of *one* individual in the winter of 1799 by Dr. C.

Nothing can be further from my wish than to attribute that to any individual which does not appear to be justly his due; and had I not been informed by the HIGHEST authority on this subject, that Dr. Lettsom had been the first to transmit to America this inestimable treasure, I certainly should not have ventured to state it in my publication. The notice of your cor-

 respondent

respondent has, however, induced me to examine a number of letters written by Dr. Waterhouse to Dr. Lettsom, and the notes of letters transmitted by Dr. Lettsom to Dr. Waterhouse, to be convinced of the truth or error of the statement I have made. From this examination I am free to confess that Dr. L. does not appear to have *first* sent the vaccine lymph across the Atlantic. I subjoin extracts from these letters, that your readers may draw their own conclusion with respect to dates. I cannot, however, forbear expressing some degree of surprise that the practice of vaccination was not followed up by Dr. Chichester: for it is rather singular, that no mention of his name occurs but in the statement contained in the Philosophical Magazine, unless it be in the writings of Dr. Geo. Pearson, which I happen not to be in possession of, and to which it is at present not in my power to refer. Dr. Waterhouse, on the contrary, is frequently alluded to by various writers. He was the active promoter of the practice in America—the person to whom the members of the Government applied for lymph and for directions for its use, and to whom also his professional brethren looked for information on this subject. He seems also, from the following extracts, justly to feel the responsibility of his situation on the occasion. If, therefore, Dr. W. was not the first (which I confess appears to me doubtful) who vaccinated in America, it is but due to admit that it was by him that the practice *spread* through, and was finally established in, the United States.

Dr. W. to Dr. L.

"Cambridge, April 10, 1799.

"I received with great satisfaction your letter of the 24th of November, with Dr. Jenner's and Dr. Pearson's publications on a new, curious, and extremely important disease. I directly threw an account of it into the newspapers, a copy of which I here inclose. I should be highly gratified by more information respecting this epizootic disorder, and of further trials on the human kind. As such a distemper has never been head of in this country, it excites the public curiosity as much as any thing that has occurred in the medical line since my remembrance."

Nov. 14, 1799. "I here inclose a letter for Dr. Woodville, as it is making the same request I did to you respecting some cow-pock matter*. I send it opened, and wish you would be so kind as to put a wafer in it and suffer the penny post to convey it to him. The curiosity, nay anxiety of the public, especially of parents, on this subject is very considerable. We indeed feel

* From this passage it seems probable that Dr. W. had previously solicited vaccine lymph from Dr. L. No letter, however, containing a request of this kind is preserved.

anxious

anxious ourselves, as four of our six children have never been inoculated."

Nov. 13, 1800. "I am in no small tribulation for want of the vaccine matter. I introduced it into this country; but some how or other it has depreciated in my hands. It fails in more than half I inoculate for several weeks past. I never received any but from Dr. Haygarth, which was last June.

"I have never been able to procure Woodville's last publication on the cow-pox. Are there any good practical treatises recently published on this subject? As I was the first who introduced it here, I am applied to from all quarters, but am chagrined almost to sickness because I have no confidence in the matter I possess. The vaccine poison has become milder by passing through a number of the human species, or else the cold weather has deprived it of half its venom. As soon as I receive fresh matter from England, I will directly inoculate a cow, by way of obtaining active matter from the fountain head.

"I have had several instances in the cow-pox where the symptoms came on pretty violently in twenty-four hours. In many instances I am puzzled to know whether the patient has really gone through the disease so as to secure him from further infection. My situation is peculiarly perplexing; for should any unfortunate case occur under any practitioner, I shall bear the blame of it. I have diffused the matter all over the country, and am conscious that it has degenerated and become spurious. Applications by letter and otherwise crowd upon me every hour and almost every minute, to solve doubts, give directions, and console disappointments; and I have no person to apply to myself for the information which I feel I myself stand in need of. I have Jenner's work, first and second part; Woodville's first publication, and Pearson's first pamphlet, and the second volume of the Medical and Physical Journal, and could wish that Mr. Mawman would send me any thing and every thing that has or may come out in the course of the winter *which you can recommend.*"

Dec. 13, 1800. "As I know not Dr. Jenner's address, I have inclosed a letter which I would thank you to forward to him as soon as possible I have written to him on a subject in which I am deeply interested, I mean the cow-pox. You already know, perhaps, that I introduced that distemper here, and led the way in its inoculation, and that very much to my advantage; but it has lately worked very perversely, and occasioned me much perplexity. Since the cold raw weather of November came in, the *matter* has *deteriorated* in my hands, and in the hands of every one else, so that almost all the cases that have lately occurred have proved *spurious;* and unless I can ob-

tain a fresh supply of the vaccine matter early in the spring, the inoculation for it will sink into disrepute, and I myself come in for a large portion of the disgrace. Judge of my anxiety when I say, that I am conscious that more than an hundred practitioners in different parts of New England are at this time inoculating with *spurious* matter, while the small-pox is pervading one of our sea-ports, and every unfortunate case will be traced up to me the originator of the practice in America. I am the only person who ever succeeded in obtaining efficient matter from England, which I had from Dr. Haygarth, and the activity of this matter *seems worn out by passing through a number of the human species;* and unless I obtain a fresh supply from England, the business which promised so fair here will stagnate. I have, therefore, written to Dr. Jenner for his advice and assistance. So I have to Dr. Pearson, and hope, by the return of the Galen, or by the Minerva, to get a supply of the matter I stand so much in need of. I have just received an order from the War Department, to supply the military surgeons with the vaccine matter, and directions for inoculating the different corps of artillerists and engineers employed in the various parts of New England."

Dr. L. to Dr. W.

London, Mar. 28, 1800. "Sent him fresh cow-pox matter."

Dec. 24. "Sent him vaccine matter in a glass bottle, from the Vaccine Institution, as well as two planes of glasses, one from Dr. Woodville and the other from Mr. Johnson. Explained Dr. Woodville's, Dr. Pearson's, and Dr. Jenner's opinions, respecting the wearing out of the effect of the matter."

Feb. 19, 1801. "Sent him vaccine matter in a glass vessel from myself, with a letter and vaccine matter from Dr. Pearson."

I have honour to be, sir, your very obedient servant,

Bolt Court, Fleet Street, April 16, 1817. T. J. Pettigrew.

LXX. *On a new Mode of preparing arsenicated Hydrogen Gas; with the last Experiments of* Gehlen *on this Subject.*

[The experiments which this paper announces, possess a peculiar interest, from the melancholy fate of the experimenter, M. Gehlen, who fell a victim to the deleterious effects of the gas, the constituents of which he was analysing. The account of them which follows, has been made up from the memorandums he left behind him; and the observations which are added are from the pen of the ingenious M. Gay Lussac*.]

In order to ascertain how arsenic is affected by caustic potash, M. Gehlen put 200 grains of arsenic along with three times as much

* From the *Annales de Chimie et de Physique,* for October 1816.

much of the lye of caustic potash of a specific weight, in a distilling apparatus fitted to collect the gas. At first only the air of the vessels was disengaged; nor was any gas collected until the greater part of the water had evaporated and the lye had thickened and begun to bubble up. A disengagement of gas then commenced, which continued with rapidity until the mass was dried. The gas which was obtained had no smell, and burned with the slight and scarcely perceptible flame of hydrogen gas. The residue in the retort occupied a large space; when withdrawn it was found to be spongy, of a deep red brown in the inferior part, and inclining to black in the superior, in the cavities of which some small octagonal crystals of arsenic were observed. The mass quickly attracted the humidity of the air, and its colour changed from a red to a blackish brown. When water was thrown over it, it dissolved quickly, became heated, and displayed a rapid ebullition, which however soon ceased. The gas which developed itself had the smell of garlic, somewhat resembling that of hydrosulphuric gas. After all the pieces of glass were separated, and the finest of the crystals of arsenic washed, the liquid part which was very alkaline was filtered, and the residue washed upon the filter.

Observations.

I have repeated the experiment of Gehlen, and have found that it has been described with much exactness;—I would only add some remarks.

The dissolution of potash ought to be perfectly caustic. It is only, as Gehlen has observed, when it is very concentrated that it acts upon the arsenic; the gas, which then disengages itself in abundance and with rapidity, is nothing else during all the time of its disengagement but pure hydrogen gas; it does not deposit any arsenic during its combustion, and consumes exactly half a volume of oxygen.

When, instead of arsenic, its oxide is employed, it also gives out a good deal of hydrogen, but only when the potash is nearly desiccated and the temperature elevated. The result of the decomposition of water by the potash and the oxide of arsenic is an arseniate of potash, which may also be obtained by heating together the oxide of arsenic and carbonate of potash fused and deprived of water. In this case the carbonic acid is given out, and one portion of the oxide is reduced to acidify the other.

The mass of a deep red brown resulting from the action of the caustic potash upon the arsenic appears to me to be a mixture of arseniate and arseniuret of potash. It is not likely that it can consist of arsenite; since, when the oxide of arsenic acts upon potash or upon its carbonite, it produces an arseniate.

The

The arseniuret, which seems to me quite analogous to the alkaline phosphuret, decomposes water as soon as it comes in contact with it, and from this decomposition results arsenicated hydrogen gas, which disengages itself, because, similar to phosphuretted hydrogen gas, it does not possess any acid properties. I believe it, too, to be identically the same with the gas which is obtained by heating the arseniuret of tin with hydrochloric acid. One portion of liquid which was very alkaline, being filtered and supersaturated with hydrochloric acid, yielded but a very slight yellow precipitate with hydrosulphate of potash, and only discoloured a very small quantity of the red sulphate of manganese. Another portion of liquid was saturated with acetic acid, then evaporated to dryness, and treated with very concentrated alcohol. The acetate of potash was found to predominate in the base, although the two salts were originally neutral, and the residue consisted of arseniate acid of potash. A similar division was obtained by evaporating neutral arseniat of potash, which never crystallizes so well as when combined with an excess of acid, and then it is analogous to tartrate of potash.

It is remarkable enough that hydrogen gas does not combine with arsenic when potash is made to act on that metal, while it combines very well with it when the arseniuret of tin is decomposed by hydrochloric acid. I apprehend that this difference is owing solely to the temperature, which is much greater in the first case than in the second; for I am certain that by a spirit of wine heat arsenicated hydrogen gas may be decomposed. It is nearly the same with carbonetted hygrogen gas;—if it is prepared at an ordinary temperature it contains a considerable quantity of carbon, and very little or none at all when obtained at an elevated temperature.

In the preparation of phosphuretted hydrogen gas by potash, a gas is often obtained which has not the property of inflammability when in contact with the air, and which probably is sometimes pure hydrogen. Is it not probable that this may also proceed from too high an elevation of temperature? For phosphuretted hydrogen gas is easily decomposed by heat, and that which is obtained by the dissolution of phosphurets in water is always more charged with phosphorus than that which is produced by heat. I may remark, that although arsenicated, phosphuretted, or carburetted hydrogen gas is decomposed at a temperature which may be supposed little different from red heat, it does not follow that hydrogen gas ought necessarily to combine with arsenic, phosphorus and carbon, at a temperature a little inferior; for there is always a distance more or less great between the temperature necessary to effect a combination, and that which is sufficient for a decomposition.

Arsenic

Arsenic acts upon barytes, but not so much as potash. By passing the vapours of arsenic over red barytes, a combination took place, the barytes being entirely penetrated with the arsenic. On adding cold water, no disengagement of gas took place; but by heat I obtained a little arsenicated hydrogen gas.

I made afterwards a mixture of subcarbonate of soda in solution and arsenic; I put it into a glass vessel to which I had adapted a tube to collect the gas, and exposed it to a red heat. The arsenic began soon to volatilize, and disengaged but a small quantity of carbonic acid, which might be owing to a little of the oxide of arsenic which always accompanies arsenic exposed during some time to the air. The subcarbonate did not appear to have experienced any alteration.

From these observations it will be seen that arsenic has great affinities to sulphur, and especially phosphorus; like them it combines with several oxides, and like phosphorus it forms with hydrogen gas a combination which does not possess acid properties. But it ought to be placed after phosphorus, because its affinities are weaker, and it does not disengage the carbonic acid of carbonates.

In making these experiments I have had occasion to remark, that hydrosulphate of potash dissolves an enormous quantity of sulphuret of arsenic. I was led from that, to endeavour to neutralize the hydrosulphate from the sulphuret of arsenic by the aid of a current of hydrosulphuric gas, but did not succeed. At the end of some days the hydrosulphate had allowed much of the sulphuret to precipitate, although in a close vessel; but for a long time it still preserved a considerable quantity. Hydrosulphuric gas has not, however, the property, like potash, of dissolving any sensible quantity of sulphuret of arsenic. I would compare the hydrosulphate of potash and of arsenic to the prussiate of potash and of zinc, which I have been equally unable to obtain in a neutral state. In general hydrosulphuric acid has a great affinity to hydrocyanic acid; but its tendency to form triple combinations is not nearly so well marked.

G. L.

LXXI. *On Animalcules,—particularly on the Polypes.*

To Mr. Tilloch.

Sir, — Should you think the following remarks upon an interesting branch of natural history worthy a place in your valuable Magazine, an early insertion of them will much oblige, sir,

Your very obedient servant,

Liverpool, April 12, 1817. John Bywater.

That

That science has received many advantages from the aid of different optical instruments is a fact universally established: but I shall now only instance them in what is termed the microscopic world; where principles and modes of animal existence have been brought to light, amongst the different tribes of animalcules, which must have remained unknown but for the aid of the microscope. This circumstance, and the great degree of obscurity which still hangs about this branch of natural history, induced me to commence an inquiry into it, thinking that the discovery of other unknown laws and modes of animal existence might reward the efforts of any one who had patience to pursue the investigation: and during this inquiry several results occurred which may not be unworthy of notice, as they may tend to remove a portion of the obscurity to which I have alluded.

If we attentively examine most of the generally received opinions respecting the different tribes of animalcules, we shall find them very unsatisfactory, as they lead to inductions which are incompatible with that simplicity and correctness of design every where discoverable throughout the works of creation.

The opinions formed respecting the *hydras*, or fresh-water polypes, are of this character; for it is stated by most writers on natural history, that polypes form a link in the chain of created beings which unites the animal to the vegetable kingdom, an inference resting chiefly on the supposed fact that they possess properties of both the animal and vegetable nature. That they possess several of the animal functions is very evident, by their power of moving from one place to another, and of swallowing and digesting their food: and that they have strong marks of the vegetable nature is equally certain; for when one of them is cut into several slips or pieces, each piece in a little time becomes a perfect being like the parent or stock from which it was taken, —a result which seems to establish its vegetable character: but if we attend minutely to several other circumstances, we shall find that this reasoning will lead to conclusions at variance with themselves, and incompatible with the general harmony of Nature.

In the animal frame there is a complete organization that runs through the whole being, with which the will or mind of the animal seems intimately connected; but if any part is cut off, that part will not possess this completed organization, consequently its animal functions will be destroyed. With this general view of the animal character, let us consider the various facts related of this race of beings, which forms the supposed uniting link between the two kingdoms.

One of the facts in favour of this conclusion is obtained by cutting a polype to pieces in every direction fancy may suggest; for

for each part, whatever may be its shape, becomes a perfect being in a little time. Now this is not the case, even with every part of a vegetable; for in general it is only when they are cut agreeably to certain rules that we can depend on an increase of plants;—hence the organization of these animalcules cannot be so completely connected as that of vegetables.

This opinion is supported by other facts related of this race of beings; for portions of different polypes may be united together, the head of one when brought into contact with the trunk of another quickly forms a perfect union; they will also exist and perform all their functions after they have been turned inside outwards like a turned glove; and that even if one polype be thrust into the body of another, they will unite and become one distinct animalcule. These are results not only incompatible with the exquisite formation of the animal frame, but display far less organization than belongs to the vegetable tribe. Yet how can this evident want of organization be reconciled with the animal functions they evidently do possess, which we know must depend upon a far more complete and connected formation than what obtains even among vegetables? This embarrassment gives birth to new opinions; but it is only from an extensive view of the subject, and the strictest attention to experimental results, that we can entertain a reasonable hope of being enabled to form more correct notions respecting this curious branch of natural history.

After having paid considerable attention to that class of objects termed *animalcula infusoria*, I concluded that the larger kind were portions of mucilaginous matter in which a great number of smaller animalcules resided, and that these animalcules had the power of shaping the mucilaginous masses, derived from the decaying vegetables and their own secretions, into such forms as to be able to move them by their aggregate force for all the purposes of pleasure and existence.

That these larger animalcules are composed of a number of living beings is highly probable; because when strongly illuminated by an oblique light they seem to consist of very small distinct scintillating points, and if we magnify them by a direct steady light we discover that these small points are more or less opaque: these appearances, therefore, naturally induce the inference that they are composed of a number of parts each possessing distinctly the principles of vitality. When polypes are magnified they appear covered all over with dark spots; therefore may we not conclude also, that these are small groups or single animalcules living in mucilaginous matter obtained from decayed vegetables and their own secretions, which they have the power of forming into the shape we call a *hydra* or *polype?*

That

That the whole tribe of animalcules possess animal functions, and are not merely organic particles without these powers, as Buffon and some others have imagined, is extremely evident; although there are some very peculiar phænomena attending polypes which appear irreconcileable with this idea; but perhaps these peculiarities furnish the best means we have of ascertaining the real nature and character of this curious race of beings. That they possess animation we have the strongest evidence, and that they have qualities corresponding to the vegetable character seems equally certain; and in my former remarks I have endeavoured to show that their organization as a mass is not even so complete as that of vegetables: therefore these seemingly irreconcileable principles, and the contrary results that attend them, force upon us the conclusion, that they are merely portions of mucilaginous matter, in which a great number of smaller animalcules reside, and that these masses owe their organic character and animal functions, as well as their curious varieties, to the influence of these little creatures.

Thus we obtain an easy solution to the wonderful fecundity these beings display in their powers of reproduction; for, if they are congeries of smaller animalcules, it readily explains why each part when cut off from the rest should possess this reproducing power; for each part so cut must contain a number of these smaller animalcules; consequently they will possess the same functions as the collection from which they were taken, and will instinctively exert their energies in giving to the portions of mucilaginous matter to which they belong all the characters the original mass possessed, which are doubtless the most appropriate to their capacities and enjoyment.

The experiments and observations which led to these conclusions were made during the last summer; but in the following autumn and winter I obtained other results more decidedly in favour of this transforming principle. About the latter end of September I procured some water from a stagnant pool covered with a large quantity of green matter, which appeared to be a species of the *conferva;* this I anticipated would furnish a great number of interesting objects for the microscope. Upon examining a part of this water I found it crowded with a vast number of the *enchelis,* or small portions of green matter endowed with a degree of animation; and agreeably to what I have supposed, they evidently derived this animation from the small animalcules they contained, which have generally been mistaken for intestines. These portions when seen distinctly appear rather more elliptical than an egg, and each end is nearly transparent; but generally the middle part seems the abode of about twenty or more smaller animalcules, which swing from side to side when these

these aggregate animalcules are in motion, and are clearly the means by which they move from one place to another. When the dark points or smaller animalcules increased in number, the middle of these mucilaginous masses, which I have termed aggregate animalcules, swelled out till they became almost round; at the same time losing a great share of their activity. It was in this inactive state they united in large quantities, and floated without motion on the surface of the water; yet they remained in this apparently inanimate state but a short period; for this matter gradually lost its green vegetative appearance, and became a brown mucilaginous body, which assumed new characters.

It was out of this mucilaginous matter that numerous aggregated animalcules of various kinds derived a more animated state of existence; but the gradual change of a large portion of it into different clusters of the polypes termed *vorticellæ*, rendered this part of the process highly interesting, and strongly supported the idea that all the *animalcula infusoria* are aggregations of small animalcules living in lumps of mucilaginous matter. Amongst other brown mucilaginous portions obtained from different quantities of water, one in particular did not give out any of the *vorticellæ*, but an abundance of distinct large animalcules, which moved about with the greatest facility;—may we not therefore justly infer that, though their act of separation from the brown mass was not observed, they, as well as the whole tribe of what are called animalcules, are generated by a similar principle to the *vorticellæ?*—an instinctive power which these dark molecules or smaller animalcules possess of giving to various parts of decayed vegetables and their own secretions all that variety of shape and character which belongs to this part of the animal kingdom?

In the various observations I made on stagnant water during the winter, I found that the green vegetative appearance which seemed to reside in the mucilaginous part of what I have termed aggregate animalcules, varied with the weather; for when it became mild this greenness increased, but as quickly decreased when the weather became cold, giving strong indications of the vegetable as well as animal nature of these little creatures.

The production known among the natives of the East Indies by the name of "*Lalan lout*, or sea-grass," gives a much stronger display of this two-fold character; as its green appearance is such that it is mistaken by strangers for grass; yet when caught in the hand it glides through the fingers and withdraws into the sand, leaving an impression of its mucilaginous and animated nature.

As by the principles here developed it does not appear incompatible with what are termed the vegetable and animal processes,

cesses, for them to go on and exist together,—may it not be inferred with some probability, that they exist and proceed together throughout the vegetable process? for innumerable quantities of small aggregate animalcules, similar to most I have mentioned, can be obtained from all young vegetable matter when subjected to infusion.—I hope this suggestion will induce others to turn their attention to these interesting phænomena: for it is only by a more strict and extensive inquiry into this branch of natural history, that we can expect to arrive at clear and satisfactory conclusions.

LXXII. *On a Coffee-Simmerer.* By Dr. Carey.

To Mr. Tilloch.

Sir,—The use of *coffee* becoming every day more extensive in this country, I presume that any suggestion, for the improvement of that pleasing and salubrious beverage, cannot be unacceptable to the public. Under that persuasion, I beg leave to communicate a method of coffee-making, which I have long practised, and which I find to answer my purpose better than any other; though I have tried several, and bestowed on the subject a share of attention, which your readers will hardly deem censurable, when apprised that coffee has, for the last three years, been my *only* beverage, except morning and evening tea.

My process, sir, is that of *simmering* over the small, but steady flame of a lamp—a process, at once simple, easy, and (without watching or attendance) uniformly productive of an extract so grateful to the palate and the stomach, as to leave me neither the want nor the desire of any stronger liquor.

But, to accomplish this, a vessel of peculiar construction is requisite.—Mine is a straight-sided pot, as wide at top as at bottom, and inclosed in a case of similar form, to which it is soldered air-tight at the top. The case is above an inch wider than the pot—descends somewhat less than an inch below it—and is entirely open at the bottom—thus admitting and confining a body of hot air all round and underneath the pot.—The lid is double; and the vessel is, of course, furnished with a convenient handle and spout.

In this *simmerer*, the extract may be made either with hot water or with cold. If intended for speedy use, hot water will be proper, but *not* actually *boiling:* and, the powdered coffee being added, nothing remains, but to close the lid tight, to stop the spout with a cork, and place the vessel over the lamp; where it may remain un-attended and unnoticed, until the coffee is wanted

wanted for immediate use. It may then be strained through a bag of stout, close linen, which will transmit the liquid so perfectly clear, as not to contain the smallest particle of the powder.

The strainer is tied round the mouth of an open cylinder, or tube, which is fitted into the mouth of the coffee-pot that is to receive the fluid, as a steamer is fitted into the mouth of a saucepan; and, if the coffee-pot have a cock near the bottom, the liquid may be drawn out as fast and as hot as it flows from the strainer.

If the coffee be not intended for speedy use, as is the case with me, who have my simmerer placed over my night-lamp at bed-time, to produce the beverage which is to serve me, the next day, at dinner and supper; in such case, cold water may be used, with equal or perhaps superior advantage; though I have never found any perceptible difference in the result, whether the water employed was hot or cold. In either case, it soon begins to simmer, and continues simmering all night, without ever boiling over, and without any sensible diminution of quantity by evaporation.

With respect to the *lamp*—although a fountain-lamp is undoubtedly preferable, any of the common small lamps, which are seen in every tin-shop, will answer the purpose, provided that it contain a sufficiency of oil, to continue burning bright during the requisite length of time.—The tube, or burner, of *my* lamp, is little more than one eighth of an inch in diameter: and this, at the distance of one inch and three quarters below the bottom of the pot—with the wick little more than one-eighth of an inch high—and with *pure spermaceti* oil—has invariably performed, as above described, without requiring any trimming, or other attention, and without producing any smoke; whereas, if the wick were too high, or the oil not good, the certain consequences would be smoke, soot, and extinction.

One material advantage,attending this mode of coffee-making, is, that a smaller quantity of the powdered berry is requisite to give the desired strength to the liquor.—The common methods require that the powder be coarse; in which state, it does not give out its its virtue so completely, as if it were ground finer: but, in this process, it may be used as fine as it can conveniently be rendered; and, the finer it is, the smaller will be the quantity required, or the richer the extract; as I have agreeably experienced, since I have been enabled, by the new invention of Messrs. Deakin and Duncan of Ludgate Hill, to have my coffee at once reduced to the proper degree of fineness, by a single operation, without the tedious labor of a second grinding, with the mill tightened. I am, with due respect, sir,

Your obedient humble servant,

West Square, Lambeth, April 2, 1817. JOHN CAREY.

LXXIII. *Analysis of a vegetable colouring Matter.* *By M.* Vauquelin.

M. Thouin has requested me to make an analysis of a new species of lac, which has been sent him by M. Morenas.

This substance, M. Morenas says, is found in hives constructed by little insects, at the extremities of different trees, which being entirely covered with them go soon to decay.

Dr. Roxburgh says he observed some thousands of excessively small animals overrunning this lac, and the branches to which it is attached, the greater number of them issuing from small holes which are in the surface of the hives. These insects run swift enough, but they are so numerous that they press one upon another.

The matter which forms these hives or cells has the appearance of transparent amber; in each cell there appears a little bag filled with a thickish red liquor similar to a jelly; the other part of the cell contains a white matter.

The Indians have given the name of *lackscha* to these cells, because of the innumerable quantity of small insects which they inclose—a lack signifying a hundred thousand.

This lac has a red purple colour approaching to violet; it has no taste, but it has a smell of amber similar to that of ants. It is neither soluble in water nor in alcohol; yet with the aid of heat a very slight violet colour may be drawn from it.

The acids, and especially the sulphuric and muriatic, mixed in water, dissolve this lac very easily, and convert its colour into a bright red. During the dissolution of this matter in the acids, an effervescence is produced by a small quantity of carbonate of lime which it contains. The lac may afterwards be precipitated from these acid solutions by means of the alkalies, provided no more than a quantity sufficient for exactly saturating the acid is used. The liquor still preserves after this precipitation a slight reddish colour.

The alkaline carbonates dissolve still more promptly and easily this colouring matter; the colour which the solution presents is a beautiful violet. As the alkalies precipitate this matter from the acids, so do the acids precipitate it from the alkalies. Flax, silk, or cotton, plunged into solutions of this lac, either in acids or in alkalies, do not take any colour, unless prepared by suitable mordants.

I have dyed flax of a very beautiful red approaching to scarlet, by plunging it in an alkaline solution of the lac, after having prepared it with a hot solution of muriate of tin, and by turning it by little and little in a mixture of the muriatic acid

with

with water. Silk I have also coloured by the same process; but the colour was not so rich,—it was that of the *hortensia* rose.

I have since proved the quality of the colour thus applied to flax and silk, both with soap and with slight acids, and I have remarked that it does not detach itself. The only effect produced is, that the acids make the colour pass to a red more lively, and more like scarlet, and the soap on the contrary makes it turn to violet.

Flax and silk coloured with this substance preserve, even after being dried, the odour of amber which distinguishes the lac.

The solution of colouring matter in the alkaline carbonates is precipitated by solutions of tin or of alum into beautiful lacs, which are either red or violet according as the liquor remains acid or alkaline after the precipitation. These lacs appear to me very substantial.

I cannot doubt, after the experiment I have made, slight as it is, that the colouring matter in question may be employed with success both for dyeing and painting. It would, however, be desirable to procure a quantity sufficient for making greater and more varied experiments than the small quantity which I have had has enabled me to make.

As the carbonate of potash (ordinary potash) dissolves it with the greatest facility, and as this alkali is every where to be procured at a cheap rate, I think it the best which can be employed for extracting the lac from the hives in which it is contained.

It would be necessary to dissolve the potash in three or four parts of water, and to boil the hives in it: when the solution has taken place, the liquid ought then to be strained through a cloth.

The solution should afterwards be desiccated by means of a gentle heat, so as not to destroy the colouring matter, and then put into vessels for transportation.

When wished to be used, it will be sufficient to dissolve this compound in water.

LXXIV. *Answer to Mr.* Farley; *with cursory Remarks on the Blow-pipe and Safety-lamp.*

To Mr. Tilloch.

Sir, — I am sorry that I have only this moment for the first time seen the last Number of The Philosophical Magazine and Journal. Mr. J. Farley, I find, has there introduced my name in relation to an oil obtained from "oil of wine." I must consider that his animadversions are at once precipitate and uncalled for.

 My

My remarks in reply shall be concise—indeed any notice from me verges on superfluity.

I simply alluded to the residuum of the oil of wine when the ethereal vapour was expended in the minimum combustion detailed; I did not exalt it to the merit of a discovery, but indirectly alluded to it as a thing the knowledge of which was not confined to me. The terms were general, and Sir H. Davy might as well question the allusion made to the new acid gas. I have never once said that it occurred to me, individually, in the prosecution of experiment.

It cannot be my wish to deprive Mr. J. Farley of all the advantages which may result from his mighty discovery of the "peculiar fixed oil." For my own part, it is, I think, with the title of *discovery*, too splendidly adorned. However, we shall no doubt receive a masterly analysis of its chemical constituents from this gentleman; until which period, I shall certainly hesitate to rank as a new body what only "seems" to be "a peculiar fixed oil," from a superficial and cursory examination of it.

I cannot charge my memory at present with any conversation with Mr. Farley relative to this substance, except that he told me at one time, it was *heavier* than water, and at another period, *lighter*. In the first instance, I distinctly told him that I had tried platinum in oil of wine. Certainly, if a person has made *a discovery*, he has a right to publish it—My paper only detailed "the phænomena of platinum and other wires in inflammable media." I shall not envy Mr. Farley his *discovery*—I only say that he has been rash.

I read with some interest Dr. Clarke's proposition for an improvement in the oxihydrogen blowpipe, in the last Number of the Annals of Philosophy; because *you yourself* proposed to me *exactly such an appendage*, to increase the intensity of the flame, two or three months ago*. The rough sketch you gave me at the time, I have somewhere in my possession. There can be no doubt but that such a fasciculus would exalt the ignition.

I omitted to mention an experiment by which any one may satisfy himself of the success of the appendage I have presumed to recommend to Davy's lamp, by which it may relight itself. Compress a spiral wire of the thickness of a knitting needle, heat this red hot, blow out the candle and bring it over the wick like an extinguisher. It will be immediately rekindled.

* The proposal alluded to was to interpose a bundle of capillary tubes between the holder of the compressed mixture of gases, and the small tube through which it is ejected on the lamp; but as I had not applied it to use in any way whatever, or published it, but the same idea may have occurred to others, the merit belongs in justice to Dr. Clarke, who first gave it to the public.—A. T.

Your

Your reply, sir, to Mr. Stephenson is most conclusive, and will be by all generous and candid minds accepted, as fatal to the priority claimed most unwarrantably by this person. Mr. Stephenson, however, has one merit, namely, that of an *exact copyist*,—witness even the *trimmer for the lamp*. The miner will remember his "Davy" with gratitude, when the "Idea" of Stephenson will be consigned to native "nothingness." The beings of former ages could never have anticipated the happy period when the miner should encounter a magazine of fire-damp, in full consciousness of security, and, as Lord Erskine well expresses it, "with Davy's magic lantern by his side."

I am with full respect, sir,

Your obliged servant,

Glasgow, April 23, 1817. J. MURRAY.

LXXV. *Notices respecting New Books.*

M. CONRAD LODDIGES, of Hackney, so long celebrated as a cultivator of plants, is preparing for the press a work called The Botanical Cabinet, which is to come out in numbers, containing coloured plates of exotic and British flowers which have blown in his garden. This will comprehend a great number of the most curious species known in Great Britain.

Mr. William Phillips, author of the Outlines of Mineralogy and Geology, will publish next month a small duodecimo volume, comprising Eight familiar Lectures on Astronomy, delivered at Tottenham last winter to a numerous audience consisting chiefly of young persons. It will contain the requisite diagrams and illustrations; and being intended for the initiation of the young, and for those who are unacquainted with the science, its numerous terms are as much as possible avoided, and such as cannot be avoided are fully explained, in these Lectures.

Mr. Thomas Forster has just published the fifth edition, with additions, of his Treatise on the Brumal Retreat of the Swallow; together with a copious Table of Reference to authors who have treated of this subject.

LXXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 17. THE Society having assembled after the holidays, the conclusion of Mr. Marshal's Natural History of the Cinna-

mon Tree was read. The true cinnamon tree rises to the height of thirty feet; its roots yield camphor; its leaves are seven or eight inches long and two or three broad; its flower is white, and of a most disagreeable odour; but its berries are greedily devoured by the birds. What is called *cassia* is the receptacle and unripe seeds of the *laurus cinnamomum*. In Ceylon there are four cinnamon plantations containing from 1000 to 3000 acres each: three of them are represented as being well cultivated, and the fourth in a rather decayed and unproductive state. The Chinese, it appears, were the great traders in cinnamon; but, according to Ribiera, there is no account of their commerce in this article before the ninth century. Mr. M. thinks that the term scinnamon and cassia are of Malay origin, and that the tree was originally cultivated for its bark by the Malayans.

Mr. Knight communicated a supplement to his former paper On the Binomial Theorem, in which he acknowledges that the method he proposed is found in the work of the late Mr. Spence, which he had not seen till very recently.

C. Babbage, Esq. F.R.S. communicated an ingenious paper on the use of analogy in mathematical reasoning; but the greater part of it was of a nature unfit for public reading.

April 24. A paper by Mr. Uppington, describing the nature and advantages of an instrument which he calls an "electrical increaser," was communicated by Dr. Pearson, and read. It consists of a series of brass plates which carry and retain the electric fluid; but as their construction was exhibited by drawings, it is impossible to convey a distinct idea of this apparatus. The details of the experiments were originally communicated in letters to the late Lord Stanhope.

ROYAL SOCIETY OF EDINBURGH.

On the 13th of January the annual election of office-bearers took place, when Lord Glenlee was chosen one of the Vice-presidents in the room of the late Lord Meadowbank; and Professor Jameson, Colonel Emery, Dr. Macknight, and Professor Dunbar, Counsellors, in the room of Walter Scott, Esq., Dr. Jamieson, Dr. Brewster, and Mr. Brice, who went out by rotation.

Since our last Report, the Rev. Mr. Allison read a second part of his Biographical Account of the late Alexander Fraser Tytler, Lord Woodhouselee, and his writings.

A paper by Mr. Thomas Lauder Dick, on the appearances called the "Parallel Roads" in Glenroy, in the shire of Inverness. This glen extends about eight or nine miles from NE. to SW. and consists of six or seven distinct vistas or reaches, produced by the projections and bendings of the hills. It is very narrow, and the river Roy runs along its bottom. On the sloping sides of the

the hills on each side are seen what have been called the Parallel Roads,—a series of shelves receding one above another, through the whole extent of the glen. In most parts they are three in number, in some parts only two: in one place five are distinctly perceptible. Each shelf preserves a horizontal position throughout the length of the glen. The second road is about thirty yards lower than the first, and the third about sixty lower than the second. In number, height, and position, they are similar on the opposite sides of the glen. Their surface inclines outwards in a slope of about one foot in five, and their greatest breadth is about twenty yards, but in many places much less where the materials of the hills are hard.

These shelves, which some have supposed to be artificial, Mr. Dick shows, very satisfactorily, must have been produced by the action of the surface of a vast lake, which must have filled the valley, but undergone a series of successive subsidences, by the bursting out of its waters, corresponding to the number of "roads" now visible. He has, he thinks, ascertained the point in the glen through which the waters rushed when the lake subsided to the second level.

Mr. Dick supports his theory by observations made on the margins of deep lakes in the Highlands, and by an analogous road or shelf, which surrounds a valley above the town of Subiaco, forty-six miles east from Rome, and which is known to have been once on a level with the waters of the lake, by the ruins of the baths of Nero, and of the aqueduct by which Appius Claudius conveyed water from this lake to Rome, though the lake is now much lower.

Dr. Brewster communicated experiments made by himself and Dr. Gordon on the human eye, relating chiefly to the refractive power of the aqueous, vitreous, and crystalline humours, and to the polarizing structure of the different parts of this organ. Contrary to the received opinion, the aqueous and vitreous humour were found to have refractive powers greater than that of water, that of the vitreous humour being the highest. The crystalline lens exhibits a polarizing structure exactly the same as quartz, or one set of doubly-refracting crystals, or the same as the middle coats of the crystalline lens in fishes (Phil. Transac. Lond. for 1816, p. 311.). The iris has the same structure, but the cornea had an opposite structure, nearly the same as that of calcareous spar; or the same as the outer and inner coats of the crystalline lens in fishes. The tint polarized by the crystalline of the human eye is a faint blue of the first order.

A paper by Dr. Craigie was read, on the affinity between the Persian and the Greek and Latin languages.

A letter from T. Allan, Esq. gave a sketch of the mineral

structure

structure of the country round Nice. It is chiefly composed of limestone, disposed in irregular strata, containing shells of the same description with those of the sea beneath.

Sir George Mackenzie read an essay On the Theory of Association in Matters of Taste. It is of considerable length, and occupied the whole time of three meetings of the Society.

SOCIETY OF ANTIQUARIES OF LONDON.

On the 23d of April, being St. George's Day, the Society of Antiquaries met at their apartments in Somerset-place, in pursuance of their statutes and charter of incorporation, to elect a President, Council, and Officers of the Society, for the year ensuing; whereupon

George, Earl of Aberdeen,	Henry Ellis, Esq.
Right Hon. Sir J. Banks, Bart.	Hugh Leycester, Esq.
F. A. Barnard, Esq.	Samuel Lysons, Esq.
William Bray, Esq.	Mat. Raper, Esq.
Nicholas Carlisle, Esq.	Robert Smirke jun. Esq.
Taylor Combe, Esq.	

eleven of the Council, were re-chosen of the New Council; and

Samuel, Lord Bp. of Carlisle,	Thomas Murdoch, Esq.
Hon. Rev. H. C. Cust,	Sir John Nicholl, Knt.
George Dance, Esq.	John Rennie, Esq.
Thomas Lord Grantham,	Colonel R. E. Roberts,
Charles Hatchett, Esq.	Rev. S. Weston,

ten of the other members of the Society, were chosen of the New Council, and were severally declared to be the Council for the year ensuing.

And, on a Report made of the Officers of the Society, it appeared, that

George, Earl of Aberdeen, was elected President;
William Bray, Esq. Treasurer;
Taylor Combe, Esq. Director;
Nicholas Carlisle, Esq. Secretary; and
Henry Ellis, Esq. Secretary for the year ensuing.

The Society afterwards dined together at the Crown and Anchor Tavern in the Strand, according to annual custom.

BATH LITERARY AND PHILOSOPHICAL SOCIETY.

March 17. Mrs. Grose favoured the Society with some specimens of the *Cicada mannaferens*, or locust of New South Wales, and likewise of the wild honey or manna deposited by that animal on a large forest tree called the *Eucalyptus*. This insect continues but a short time in its winged state: it was first observed in November 1800, by Colonel Paterson, in the pupa state, and on the same day it appeared with its wings through an opening in the back of the outer covering; it was then in a

very

very weak state, and slowly left its original abode. The rapidity with which the insect enlarges after this, is surprising; in the course of a few hours it can fly to the top of the tallest *eucalyptus*, which generally grows to the height of sixty or seventy feet. On this tree Colonel Paterson first discovered the manna in great quantities, apparently produced by these insects. It may be collected both in a liquid and in a saccharine state: the inhabitants gathered it, and used it for some time as sugar, but soon discovered that it possessed in some degree the quality of manna. The extraordinary noise these little creatures make is deserving of notice: the males first begin with a note similar to that of the land-rail, and repeat it for several times; at length the females join, when the combination of notes exactly resembles the noise of grinding knives or razors; and hence the insect is popularly known by the name of the razor-grinder. It makes its appearance about the end of November, and early in January deposits its eggs in the ground. The larva is perfect in September, when it is formed into the pupa, in which state it remains until November. There is a species of the insect in New South Wales of the same appearance, and which makes the same sort of noise, but produces no manna.

Mr. Eckersall observed that the locust, when used as food, contracts the usual period of life, and induces that cutaneous affection of winged insects generated on the surface of the body, producing universal ulceration.

Dr. Wilkinson stated that he had received a letter from Mr. Bakewell the geologist, communicating that, from some observations on basaltic formations, he was led to believe that the basalt makes its appearance at Wick and dips under Bath, where it has some connexion with the production of the warm springs.

The Society proceeded to the consideration of a paper formerly presented by Dr. Wollaston relative to the theory of the diamond cutting glass. It was stated by Dr. Wollaston, that the natural point of the diamond answered better than an artificial point; and that, in cutting glass, the line described is a tangent to the face of the diamond. Dr. Wilkinson mentioned that he had some micrometers made by the late Mr. Coventry, where the lines on glass had been so finely drawn, that the cross lines formed a series of squares so minute that twenty-five millions of squares are only equal to one square inch. Dr. W. observed, that when a glass is cut properly by a diamond, the line of section has a very different appearance from a line drawn by an improper inclination of the point; the particular difference he promised to state at a future meeting. It was conjectured by one of the members, that the effect of the action of the diamond may not be a mechanical

chanical section, but a partial solution of continuity effected by a vibratory motion.

A member mentioned, that it had been publicly stated to be in agitation, to contrive an apparatus by which 1000 gas lights might at once be lighted by electrical means. Dr. Wilkinson said that he did not deem any electrical apparatus at present existing, competent in its highest state of excitement to produce such an effect. Supposing the wires at each light one-tenth of an inch distant from each other, the sum of all the striking distances would be equal to one hundred inches, when a thickness of plate of air to be broken through would require a plate machine twenty feet in diameter, the glass part of which could not be made for 100,000*l.*

Dr. Wilkinson next directed the attention of the Society to the application of his mechanical theory to the phænomena of light. He referred to the change of direction in a ball when obliquely impelled out of air into water, and this he denominated the refraction of the ball: however inclined, the ratio of deviation is constant and uniform, depending on the difference of resistance between the two media. The same principle Dr. W. applied to the refraction of light. He considers all transparent bodies with respect to light, the same as conductors with respect to electricity; that is, that the transparency depends on the universal diffusion of light with the transparent substance, and that the first portion of light which is rendered apparent, is that which previously existed in the substance impelled by the superinduced quantity.

April 7. A paper by Mr. Eckersall, on Locusts, having been read, Mr. Chapman favoured the Society with some remarks in opposition to the theory advanced in it by that gentleman. He objected strongly to the supposition of Mr. E. that any ova which might be conveyed into the stomach through the medium of the locusts, when made use of as food, could afterwards force their way to the skin, so as to form that cutaneous disease represented by Diodorus Siculus and other writers, ancient as well as modern, to be so very dreadful in its effects. The President observed, that the cutaneous disease was more probably a consequence of constitutional debility occasioned by the use of a food containing so little nourishment.

The plan of a new drag or creeper, for searching for drowned bodies, was presented to and approved of by the Society. It consists of an iron rod, at least six feet in length, divided into three parts by two joints; so that (the sides of rivers being generally sloping) the two extremities of the rod may lie on either bank, while the central part keeps its horizontal position on the bed of the river; to which rod are attached a number of creepers

at

at the end of small chains about one foot asunder. This instrument towed by a small boat, will, it is thought, completely search the bed and banks of any small river.

Mr. Rotch favoured the Society with the sight of a beautiful glass flute, made in Paris, of very superior workmanship and tone. Mr. R. expatiated at some length on the superiority which glass possesses over every other substance for the propagation of musical vibrations, owing to its peculiar elasticity, and the high polish of which it is susceptible.

April 14. Mr. Ricardo having directed the attention of the Society to the melancholy case of imputed murder by poison, which occurred lately at Falmouth, and to the conflicting opinions of the medical men examined on the occasion; Dr. Wilkinson remarked, that in all cases where arsenic is suspected, it is unsafe to depend on any one apparent proof of its presence; and that it is only from a combination of a great many unequivocal proofs that a sound inference can be drawn. Although the ammoniated nitrate of silver applied to a solution of arsenic produces a lemon-coloured precipitate, yet the shade so nearly corresponds with that arising from the same test in combination with the phosphate of soda, that the one may be easily mistaken for the other. [Perhaps the only perfect and indubitable test, as stated by one of the witnesses on the trial, Dr. Neale of Exeter, is the reproduction of the arsenic by sublimation.—EDITOR.]

Dr. Wilkinson afterwards resumed the explanation of his mechanical theory of electricity. Metallic bodies, he observed, contain the largest quantity of electricity, and non-conductors the least, the quantum being always proportionate to the capacity of the substance. Thus sponge, wood, and marble, when immersed in water and then removed, produce equal effects on the hygrometer. The quantity of water each retains is very different,—but it is the excess above their respective capacities that influences the hygrometer. The same law the Doctor applied to caloric, conceiving it to be only the excess of heat above the calorific capacity of the body that affects the thermometer; and that to alter that capacity some change in its constituent parts must take place.

In the case of a conductor of electricity, the Doctor supposes this principle to prevail so much, that a continued chain of communications must exist between the electrical particles. The first spark received from the prime conductor of an electrical machine he considers to be the natural electricity of the conductor driven forward by a superinduced quantity;—in the same manner as in the case of water impelled through a tube, the

portion

portion which passes out at one end is driven forward by the portion passing in at the other.

In illustration of the change produced in electrical capacity by a change in the constituent particles of a substance, the Doctor remarked how imperfect a conductor metal becomes when oxidated;—that in other substances the conducting powers are increased by an increase of temperature:—thus vapour is a better conductor than water, and water than ice, and the latter when at a very low temperature becomes a non-conductor.

The quantity of electricity existing in the atmosphere, Dr. W. considered to be principally regulated by the proportion of vapour distributed through the air, dry air being known to be an excellent non-conductor; and to this resistance which electricity experiences with respect to air, the Doctor ascribed the development of its true principles. Were air a conductor, no electrical changes could occur, since there could not be any accumulation on the one part nor deficiency on the other—we should, in short, be deprived of all the advantages we derive from atmospherical electricity. But as it is, action cannot take place without unfolding a portion of this enlivening principle. The evaporation of a drop of water, equally with the concourse of floating fields of vapour, elicits more or less electricity; and it is from the process of equalization which takes place when showers descend on plants containing different proportions of electricity, that that action proceeds which excites and stimulates vegetation.

Dr. W. concluded by observing that he should apply these principles to the explanation of the Leyden phial at the next meeting.

LXXVII. *Intelligence and Miscellaneous Articles.*

EXPLOSION OF AN ENGINE-BOILER.

On Friday morning the 4th of April, a most lamentable occurrence took place at Norwich. Just after the steam packet, bound from that place to Yarmouth, had started, and before she had proceeded twenty yards, the boiler burst with a most dreadful explosion. The boiler was a cylindrical vessel, about eight feet long and four feet two inches in diameter, of wrought iron, excepting one end, which was made of cast iron. The latter gave way, and was propelled towards the stem of the vessel, while the body of the boiler was thrown in a horizontal direction out of the stern, sweeping all before it; at the same time that the concussion of the air and steam completely unroofed the vessel from one end to the other.

When the explosion took place there were twenty-two people on

on board, of whom only four remained unhurt. Nine were killed; and the remainder more or less wounded; and of the latter three are since dead, the engine-man, named Diggins, being one of them.

As might be expected, this event has spread a very general alarm, and proved injurious to the business of vessels employed in steam navigation; from an idea that accidents of this kind cannot be guarded against, but must be expected as a matter of common occurrence, from the nature of the moving power employed; especially if the steam engine be what is commonly called a high pressure engine, that is an engine worked with steam of forty pounds pressure per square inch, or upwards. That people ignorant of the properties and management of steam should reason thus is not wonderful, but that men who call themselves engineers should assist in propagating such errors is surprising.

Every accident of this kind may be traced either to faulty construction, or criminal mismanagement. If a boiler can be made to work safely under a pressure of four pounds per inch, it is self-evident that one made of ten, fifteen, or twenty times the strength, may be worked with equal safety under a pressure of forty, of sixty, or of eighty pounds the inch. What competent engineer would avow that he could not make a boiler sufficiently strong to resist a pressure of 200, or even 300 pounds per inch? But if the safety valve of the boiler be locked down, whether constructed to resist four pounds or 400 pounds, what can be expected but an explosion?

From all that we have been able to learn respecting the explosion at Norwich, we have no doubt whatever that it was owing to faulty construction, criminal hardihood of the engine-man, and culpable mismanagement. The boiler was made of two kinds of materials, of different expansive powers when influenced by changes of temperature: this ought never to take place. The cast-iron end was *a flat plate*, with a double circular rim at a right angle to it, like the lid of a snuff-box; this was slipped over the end of the wrought iron cylinder. Through the double rim and the interposed cylinder of wrought iron were passed numerous bolts from the inside, and these were secured by screw-nuts on the outside—that is, in such a manner that the very operation of screwing home the nuts had a tendency to fracture the cast-iron plate at the turn of its rim to a right angle. The space between the rim and the cylinder was filled with the usual cement. The plate itself was *only*, as we are informed, about *three quarters of an inch in thickness!* and it was usual to raise the steam to a pressure of *seventy pounds* the inch before starting for the voyage. At the time the explosion took place, the attendant engineer

engineer was sitting across the boiler, working home a screw into some part of the machinery, consequently not attending to the height of his steam, and possibly, from his position, preventing the possibility of the safety valve rising to allow of the escape of any steam, whatever the pressure might be; while at the same time the end of the boiler was quite incompetent to hazard such a pressure as even seventy pounds per inch; and if only a few degrees of heat were then added, the expansive force would very soon be doubled! Indeed, we have every reason to believe, that on the present occasion the steam was wilfully urged to a degree beyond all prudence. In fact, the explosion seems to have taken place from precisely the same kind of blameable and obstinate conduct which has so frequently occasioned serious mischief to passengers in stage-coaches. A rival steam-boat was started at the same time, and the engine-man of the one that exploded had been heard to declare, that he would beat the other in the voyage, and was then actually urging his steam to outrun the other boat.

But, because stupid pretenders venture sometimes to meddle with an agent with whose powers they are not sufficiently acquainted; and risque their own lives, and endanger others, by employing boilers which they have not even subjected to a cold-water proof *, would any one be so foolish as to propose parliamentary interference to put an end to the use of steam of high temperature in any of the machinery or numerous processes carried on in this country? Yet there are men, who either from stupidity, or from something more blameable, have been talking in this foolish way. As well might it be suggested, that men should be prohibited from riding on horse-back, or travelling in carriages, because numerous accidents are thus occasioned which could not possibly happen if people were compelled to walk upon their feet; orthat at any rate, if carriages must be used, they ought only to be waggons, not coaches, which by their quicker driving are every now and then breaking people's limbs, if not their necks.

Away with such quackery! No improvement of any magnitude was ever yet introduced into the machinery employed in

* Every boiler intended to be used under steam of high pressure, should be previously proved by injecting, by means of a forcing pump, cold water into it, while the safety-valve is loaded with six or eight times the weight that is to be employed when working with steam. Efficient boilers may be made of copper, of wrought or of cast iron, due regard being had to the nature and relative strength of the material. The boiler should always have two valves (one of which should be out of the reach of the attendant), and should be also furnished with what may be called a mercurial valve—an iron tube in the form of an inverted syphon; of a proper length, filled with mercury to a height answerable to the pressure to be employed.

our

our various manufactures, that was not attended with more or less risque in its infancy. Steam engines, and steam of high pressure, may be, and are, used by men of science and intelligence with as perfect safety as any of the other powers of Nature that have been made subservient to the wants or to the convenience and comfort of man.

Since the foregoing particulars were sent to the printer, we have been furnished with others which should be made known to the public, that the blame may fall where it ought.

Diggins, who was before a common labourer, was first employed to attend a steam engine in the brewhouse of Alderman Patteson of Norwich. In Mr. Patteon's service he was so obstinate and fool-hardy that his employer often reproved him, and was obliged to interfere and to make him rake out the fire.—His master was absolutely afraid at last to go into the premises.

When he left this employ, whether dismissed for obstinacy, or tired of the reproofs of his prudent master, we know not, he was engaged as engine-man to the Hope steam packet-boat. We have been informed by Mr. Watts, a respectable engineer of Norwich, that while in that boat he has frequently detected him urging the steam to 120 pounds the inch—how much higher he may have urged it at times no person can tell.

From the Hope he went to the Telegraph, and while he attended that vessel he went on precisely in the same mad manner; and when the mischief occurred was actually trying a race against a rival packet which had got the start of the Telegraph, determined either to pass her or force the Telegraph into her wake—In fact, in place of steam of 60 pounds, as has been supposed, he must from his known mode of firing have had steam of 120 lbs. or probably much higher.

The approaching explosion was intimated by a leak at the joining of the cast-iron end.—The man, alarmed by the effects of his own folly, lost his recollection, and was attempting none can tell what, and had got on the top of the boiler when it exploded.

The boiler was so defective in construction, that the only wonder is that it did not explode long before; for it was more common with him to have steam at from 100 to 120 than even at 70 lbs. per inch.

The mischief occurred precisely in the same way as many of the numerous accidents (as they are called) which befall stage coaches when foolish drivers urge their horses to an undue speed.

What person in his senses—who that is competent even to give an opinion—who that aims only at what is direct, proper, and laudable, would contend that such a case as this should be made a ground work for alarm?—a pretext for putting down the use

use of steam of high pressure in engines, or in the different manufactures of a country whose prosperity depends so much on her superiority in all the resources of ingenuity, of enterprise, and of intellect?

Who that seeks the welfare of the community would lend his aid to stop the progress of British ingenuity and industry, and compel the nation to retrograde in our various arts, by exciting and spreading among the ignorant groundless alarms, instead of fairly informing them of the imprudence and criminal mismanagement that have produced the mischief?

Are all the contrivances and processes which require compressed air to be put down, because the air vessel may possibly be burst?—They must, if the use of high pressure steam is to be exploded.

Notwithstanding any accidents that have yet happened to persons travelling by steam vessels, it does not appear that the danger to individuals is greater than in travelling by stage coaches, the mischiefs from which have become so frequent as, long ago, to have ceased to excite that alarm which is produced by such accidents as occur in the use of any powerful agent to which the public attention is called by its novelty and importance. In the nature of things, the accidents that have occurred will produce their own remedy; and any thing like Parliamentary interference would be so highly injudicious, that it is impossible to calculate or even to foresee the extent of mischief that might thus be produced to the manufactures and mining interests of the country, as well as to every landholder on whose estate valuable minerals may hereafter be discovered.

STEAM ENGINES IN CORNWALL.

The average work of 26 common steam engines, reported by Messrs. Lean for January, was 21,339,431 pounds of water lifted one foot high with each bushel of coals consumed.

Woolf's engine at Wheal Fanny, loaded 15·4 per square inch in engine cylinder, lifted 35,759,081 pounds with each bushel.

His engine at Wheal Abraham, loaded 15·1 per inch, and his other engine at the same mine, loaded 3·49, lifted 19,390,201 pounds with each bushel.

Woolf's engine at Wheal Unity, loaded 13·1 per inch, lifted 29,277,600 per bushel.

The Wheal Chance engine, loaded 13·78 per inch, lifted 45,791,866 pounds, and the Dalcouth engine, loaded 11·2, lifted 40,723,628 pounds one foot high with each bushel of coals consumed.

We have observed, in the *Annales de Chimie et de Physique* for November last, a notice respecting Woolf's engine. Alluding to these

these engines in Cornwall, the Editors describe them as those in which oil or melted tallow is interposed to prevent the possibility of steam escaping past the piston, and which have been described in our xlvith volume. The Cornish engines are not of this construction, but conformable to Mr. Woolf's first patent; and are worked by steam expanded by temperature, as described in our xixth volume.

FURTHER ERRORS IN THE NAUTICAL ALMANAC.

To Mr. Tilloch.

Sir,—In the *explanation* inserted at the end of every Nautical Almanac it is stated that "the conjunctions of the moon with the planets, or stars not less than the fourth magnitude, which may prove occultations in some inhabited parts of the globe, are evidently designed to instruct mariners or travellers to look out *frequently* for such observations: which, if they happen to prove occultations, and are carefully observed, will afford a *certain means* of determining the longitude of the place of observation." Yet, notwithstanding the declared utility of this method, and the obvious intention of the publication of the Nautical Almanac, it is much to be regretted that the Astronomer Royal (under whose directions this work is formed) should have neglected, of late years, to insert the several conjunctions of the moon with the fixed stars; whereby many persons are prevented from *looking out* for such conjunctions as may prove to be occultations at the places where they are stationed.

It is true that a *few* of these conjunctions are inserted: but, many others, which have even proved occultations in this country, are not so much as noticed. For instance, the only *conjunction* inserted in the list of phænomena for the last month (March) was β *Scorpionis*; the time of which, by the by, was set down at least an hour too late. There were, however, no less than two *occultations* in the course of the same month; viz. of γ *Virginis* on the 4th, and of η *Leonis* on the 29th; both of which were seen in London. In the *Connaissance des Tems* these conjunctions are carefully noted; and the editors of that work justly observe, that "Les occultations d'étoiles par la lune étant les phénomènes les plus utiles pour déterminer avec précision les longitudes géographiques, les voyageurs ne doivent pas négliger de les observer: les conjonctions, qu'on indique ici, serviront à les guider pour prévoir les occultations qui pourront avoir lieu dans les pays où ils se trouveront." Agreeably to these principles, a long list of conjunctions and other phænomena, occupying no less than six pages, is given in every volume of that work.

But whatever excuse may be made for omitting the bulk of such things in our National Ephemeris, every one must be sensible of the importance and propriety of having all those phænomena carefully computed which are thought worthy of being inserted: and it is to this point more particularly that I wish to call the attention of your readers. It was announced in the Nautical Almanac that, last night, there would be an occultation of α *Libræ;* and I accordingly made every preparation for observing it. But what was my surprise to find that the border of the moon did not approach within ten or twelve minutes of the star; and that consequently no occultation took place! This fact led me to suspect the accuracy of the notice for the ensuing month (May), where an occultation of the same star is announced on the 28th. It is true that an occultation of that star will take place on that day; but it will happen near an hour sooner than the time set down in the Nautical Almanac. Indeed the *emersion* of the star will actually take place above eighteen minutes before the time set down in the Nautical Almanac as the *commencement* of the occultation. So that those persons who depend on the time in the Nautical Almanac, will be deprived of the opportunity of observing this interesting and beautiful appearance.

The following comparison of the *apparent* times of the phænomenon above alluded to may, perhaps, be interesting to several of your readers.

According to the Nautical Almanac.

$$\begin{array}{lll} \text{Im.} & 9^h\ 2'\tfrac{1}{4} & \multirow{2}{*}{$*$} \\ \text{Em.} & 10\ \ 4\tfrac{1}{4} & \end{array} \quad \left.\begin{array}{r} 10'\tfrac{2}{3} \\ 3\tfrac{1}{4} \end{array}\right\} \text{So. of the moon's centre.}$$

According to my Calculation.

$$\begin{array}{ll} \text{Im.} & 8^h\ 12' \\ \text{Em.} & 8\ \ 46\tfrac{1}{2} \end{array} \quad * \quad \left.\begin{array}{r} 15'\tfrac{3}{4} \\ 3\tfrac{1}{2} \end{array}\right\} \text{So. of the moon's centre.}$$

I have already stated that the time of the conjunction of the moon with β *Scorpionis* is incorrectly stated: the same remark will apply to the conjunctions of Jupiter with the same star on the 14th of July and the 10th of August; neither of which will take place. It is evident that the computer has assumed the longitude of the star greater than it really is.

I am, sir, your constant reader,

April 4, 1817. ASTRONOMICUS.

SPECIFIC GRAVITY AND STRENGTH OF STONES.

To Mr. Tilloch.

SIR,—The article on the strength of stones, in your last number, is not correct in ascribing the experiments to Gauthey. They

They were made by Rondelet, and published by him in 1802*. They were republished in the works of Gauthey, edited by Navier in 1809†.

The specific gravity and resistance of the stones agree exactly with the tables of Rondelet, except in the instance of the pumice stone, which differs considerably. It was not to be expected that the resistance of different kinds of stones would be in proportion to their specific gravities; as the resistance of compound stones, particularly of the cemented kinds, such as grit or sandstones, must depend on the nature of the cementing material, which is frequently soft and friable; and consequently such stones, however heavy, will crumble in pieces under a moderate pressure.

The following are selected from Rondelet's work above quoted.

Names of the Stones.	Specific Gravity.	Weight required to crush a cube of which the base was 25 sup. centimetres. Kilogram.	Weight required to crush a cube of which the base was 4 sup. inches (French.) lbs. French.
Stone from Caserta in Italy: it is of a gray-white colour, fine grain, compact texture, and will receive a polish	2·718	14,865	36,142
Stone from Saillancourt. This stone is extremely hard, and was used for the bridge of Neuilly	2·408	3,536	8,680
Stone of which the theatre of Marcellus and several other buildings in Rome are constructed	2·358	7,449	18,112
Stone from one of the temples at Pæstum	2·254	5,642	13,720
Pumice stone	·675	1,053	2,520
Another specimen	·605	863	2,100
Another ditto	·556	690	1,680

T. T.

IPECACUANHA.

Messrs. Majendie and Pelletier have communicated to the Academy of Sciences at Paris an interesting discovery upon ipecacuanha. It appears that these gentlemen have succeeded in separating the principal substance to which the good effects of ipecacuanha in medicine are owing, from those adjuncts which give it that odour and taste so disagreeable to invalids. They have named this principal substance *hemetine*. A great number of experiments and observations have been made, which fully confirm the truth of the discovery.

* *Traité Théorique et Practique de l'Art de Bâtir*, tome i. liv. i.

† *Oeuvres de M. Gauthey*, tome i. p. 278 et suiv.

EXPERIMENTS WITH THE OXI-HYDROGEN BLOW-PIPE.

A letter just received from our valued correspondent M. Van Mons of Brussels communicates the following information:

"The Marquis Ridolphi of Florence has reduced earths with much ease by means of the gaseous mixture of Clarke. Brugnatelli had already made use of the same contrivance. He writes me, that he has constructed for this purpose a small apparatus, very simple, which is composed of two bladders, the one filled with phlogogene gas and the other with therm-oxygen. The tube which conducts the latter gas is of a diameter the double of that which conducts the former; and the bladders are compressed by a bar of iron. The effect is prodigious.

"M. Ridolphi has not only procured the metals from earths, but he has combined them to platinum and gold. These combinations are always white, have a metallic brilliancy, and are malleable. Being introduced into oxygenated muriatic gas or into oxygen gas, they enter immediately into a state of combustion, and form in the first case muriates, and in the second case regenerations of the earth, oxidations taking place at the same time of the metal in union. One circumstance very remarkable is, that during that oxidation it always forms itself with water, the drops of which condense upon the sides of the recipient; and this has constantly happened in spite of every care being taken to dry completely not only the apparatus but the mixture.

"The metals of earths thrown into nitric acid or oxygenated muriatic acid dissolve quickly, occasioning a hissing noise similar to that of a hot iron plunged into water.

"The oxides of cobalt, of platinum, and of gold, are reduced in an instant under the flame of the gaseous mixture; but after their reduction they inflame and are oxidized anew.

"The metals of earths decompose, in oxidizing, carbonic acid gas and water, but more slowly than the metals of alkalies.

"Of all the metals of earths, zircon is that which allies itself in the greatest proportion with platinum and gold.

"Alumine, silex, lime, and the carbonates of lime and of barytes, have been fused, but in no experiment have they yet been metallized or reduced.

"Strontian and magnesia are reduced easily, provided they are formed into paste with charcoal and oil, the paste being divided into pills and left to harden by heat. Without this preparation these earths do not fuse equally, but are fused and vitrified.

"Zircon alone deoxidizes without the aid of reducing media, and in less than fifteen minutes it appears in a metallic form.

"In these reductions the greatest effect is obtained, when instead of simple hydrogen gas, sub-carbonated hydrogen gas is employed

ployed for the gaseous mixture. The heat is then more intense, and the carbonic gas which is not consumed is of use in the reduction.

"Dr. Clarke says that under the flame of his pipe a piece of meteoric stone, which fell at L'Aigle in Normandy, was reduced into metallic iron without losing any of its weight. A piece of stone which fell at Stanner in Moravia, submitted to the same trial in the laboratory of the University of Pavia, did not experience any such effect. The thunder which forms these stones must possess at least as much intensity of heat as this new flame.

GAS FROM OIL.

Mr. J. B. Emmett of Hull has published some experiments which he made during the summer of last year, with a view of ascertaining whether a gas might not be obtained from oil, equal to that obtained from coal; so as to prevent the injury threatened to the Greenland trade by the rapidly increasing use of the latter in the lighting of towns, &c. By distilling various oils previously mixed with dry sand or pulverized clay, at a temperature little below ignition, he obtained a gas which appeared to be a mixture of carburetted hydrogen and supercarburetted hydrogen gases. This gas produces a flame equally brilliant, and often much more brilliant than that produced from coal. It differed very little in quality, whether obtained from mere refuse, or from good whale sperm, almond or olive oil, or tallow. The gas when burnt produces no smoke, and exhales no smell or unpleasant vapour. Whatever oil is used, it evolves much more light when burnt as gas than when consumed as oil; in the latter case the flame is obscured by the evolution of a quantity of soot; —in the former, the soot remains in the distilling vessel, and the flame burns with a clear light destitute of smoke.

With respect to the interest of the Greenland traders in this discovery, Mr. E. observes, that fish oil has long been banished almost entirely from private houses and shops—and that in the shape of a gas light its safety and œconomy may again introduce it into these places, and thus increase in no inconsiderable degree the consumption of oil—particularly since the gas may be rendered so far portable, that houses situated in parts of a town which are not provided with gas pipes may daily receive sufficient supplies of it without having to make it themselves.

OTTO VON KOTZEBUE'S VOYAGE ROUND THE WORLD.

The Berlin Gazette gives the following account of this expedition, which has been received from Kamtschatka. Letters of an earlier date, which, after having doubled Cape Horn, he sent from the coast of Chili, have been lost, or at least are not yet

 come

come to hand. Mr. V. Kotzebue discovered three new islands in the South Sea in 14° of latitude and 144° of longitude. To these islands he gave the names of Romanzow (the author and equipper of the whole expedition), Spiridow (an admiral under whom Kotzebue formerly served several years), and Krusenstern (with whom he made his first voyage round the world). Besides these he discovered a long chain of islands in the same quarter, and two clusters of islands in the 11th degree of latitude and 190th degree of longitude. (It is not specified whether the latitude is N. or S. or the longitude E. or W.) These he called after his ships Rurick's Chain; the two latter Kutusow's Cluster (a group) and Suwarrow's Cluster. All these islands are very woody, partly uninhabited, and dangerous for navigators. The discoverer has sent to Count Romanzow a great many maps and drawings. On the 12th of July O.S. Kotzebue designed to sail from Kamtschatka to Behring's Straits, according to his instructions. He hopes to return to Kamtschatka in September 1817. On the whole voyage from Chili to that place he had not a single person sick on board. He touched at Easter Island; but did not find the inhabitants so friendly as La Peyrouse describes them. He thinks that something must have happened since that time which has made them distrustful of the Europeans: perhaps it may be the overturning of their surprisingly large statues, which Kotzebue looked for in vain, and found only the ruins of one of them near its base, which still remains. He saw no fruits from the seeds left by La Peyrouse, nor any sheep or hogs, which by this time must have multiplied exceedingly. A single fowl was brought him for sale. It seems we may hope much from this young seaman, who is not yet thirty years of age. He was obliged for many reasons to leave the learned Dane Wormskrold behind in Kamtschatka.

SCARLET DYE.

Sir,—I should be much obliged to any of your scientific correspondents that would inform me, through the medium of your Magazine, of a method to prepare a beautiful scarlet dye of sufficient depth of colour for immediate use. I have not succeeded in its preparation by any formulæ hitherto proposed by chemists, even when the experiments were made with the greatest care and exactness, and the articles operated upon were of the utmost purity. There are two or three wholesale chemists in London, who prepare a scarlet of great durability from a substance not one quarter of the price of cochineal. These persons supply dyers with the article ready prepared for immediate use. Surely a knowledge of this substance would be a great acquisition both to the chemist and dyer.

The

The following were the results of experiments performed on the recent decoction of cochineal:

Exp. 1. Nitromuriate of tin let fall an abundant precipitate of a beautiful pink; the liquor remaining above it was almost colourless.

Exp. 2. Solution of muriate of tin produced scarcely any precipitate: the solution was of a pinkish-scarlet colour.

Exp. 3. Sulphate of copper occasioned a precipitate which formed slowly of a Mazarine colour: the supernatant liquor remained perfectly clear, having a slight tinge of the same hue.

Exp. 4. Superacetate of lead produced an abundant precipitate of a beautiful purple: the liquor above it was as clear and colourless as water. This precipitate forms a most excellent pigment for painting in water-colour.

Exp. 5. Nitrous acid digested with the decoction occasioned a strong effervescence, and an abundant orange precipitate was formed: the supernatant liquor remained perfectly limpid. This effect resembled the action between the same acid and indigo.

Exp. 6. Muriatic acid produced scarcely any precipitate. The clear solution was of a beautiful red, but dyed cloth only of a dull scarlet.

It would appear from a variety of experiments, that cochineal contains two distinct colouring principles, one red, the other purple: if so, in order to obtain a beautiful scarlet, a method must be discovered of dissolving (without altering its properties) the red colouring matter, which might afterwards be fixed by the usual mordant.

I am, sir,

Your obliged servant,

April 7, 1817. A CONSTANT READER.

*** In a preceding page of this Number we have given an article by M. Vauquelin on a dye from a new species of lac, which may be useful to our correspondent.

NEW FRIGORIFIC POWDER.

Professor Leslie, whose philosophical labours and discoveries are well known to our readers, has lately made an important addition to his curious and beautiful discovery of artificial congelation. He had found by his early experiments, that decayed whinstone, or friable mould, reduced to a gross powder, and dried thoroughly, will exert a power of absorbing moisture scarcely inferior to that of sulphuric acid itself. But circumstances having lately drawn his attention to this subject, he caused some mouldering fragments of porphyritic trap, gathered from the sides of the magnificent road now forming round the Calton-hill, to be pounded and dried carefully before the fire, in a bachelor's oven. This powder, being thrown into a wine de-

 canter,

canter fitted with a glass stopper, was afterwards carried to the College; and at a late lecture in the Natural Philosophy class (which he has been teaching this session in the absence of Professor Playfair in Italy) he showed the influence of its absorbing power on his hygrometer; which, inclosed within a small receiver of an air-pump, fell from 90 degrees to 32 degrees, the wetted bulb being consequently cooled about 60 degrees of Fahrenheit's scale. The Professor, therefore, proposed on the instant to employ the powder to freeze a small body of water. He poured the powder into a saucer about seven inches wide, and placed water in a shallow cup of porous earthen-ware, three inches in diameter, at the height of half an inch above, and covered the whole with a low receiver. On exhausting this receiver till the gauge stood at 2-10ths of an inch, the water in a very few minutes ran into a cake of ice. With the same powder an hour afterwards he froze a large body of water in three minutes, and he will, no doubt, push these ingenious and interesting experiments much further.

It appears such earth will absorb the hundredth part of its weight of moisture, without having its power sensibly impaired, and is even capable of absorbing as much as the tenth part. It can hence be easily made to freeze the eighth part of its weight of water, and might even resume the process again. In hot countries the powder will, after each process, recover its power by drying in the sun. Ice may therefore be produced in the tropical climes, and even at sea, with very little trouble, and no sort of risk or inconvenience.

HYDROPHOBIA.

M. Van Mons informs us that Brugnatelli has succeeded in curing all cases of hydrophobia by means of oxygenated muriatic acid, employed both internally and externally; which proves that in this malady the moral holds in dependence the physical powers. All cases of tardy hydrophobia may be considered as the effect of imagination. Examples have occurred of the disease reaching its last stage, when it has been completely dissipated by the sight of the animal by which the patient was bitten.

VEGETABLE PHÆNOMENON.

Among the ruins of the old monastery of New Abbey, in Galloway, there is a plane-tree about 20 feet high, which grows on the top of a wall built with stone and lime. Being straitened for nourishment in this situation, many years ago it shot forth roots into the open air. These neither died nor drew back, but descended by the side of the wall, which is ten feet high. It was several years before they reached the ground; during which time they

they conveyed no nutriment to the tree, but were supported by it. At length they dipped into the earth, and have since enabled the tree to grow with vigour. Between the top of the wall and the surface of the earth they have never thrown out either branches or leaves, but have coalesced round the wall into a sort of trunk, pretty thick, and of considerable compactness.

ORNITHOLOGY.

A few weeks ago a pair of grosbeaks (*Loxia Cocothraustes*) were shot in a field belonging to Mr. Beaufoy at Upton Gray in Hampshire. This bird is very uncommon in the south of England, and was probably attracted into the neighbourhood of Upton Gray by the plantations of pine and fir. The above two specimens were shot out of a small flock consisting of six or seven.

LONGEVITY.

The following circumstance may be interesting to those who inquire into the causes of longevity :

A gentleman of considerable research lately made a catalogue of near eight hundred persons who had attained a great age, and found their habits of life only to agree in one particular, namely, early rising in the morning. This confirms the well-known result of a similar inquiry made by one of our learned Judges.

COMPARATIVE NUTRITIVE SUBSTANCES.

A table with a sliding scale, exhibiting the proportionate nutritive powers, and intended to answer an indefinite number of questions, chiefly for agriculturists, similar to Dr. Wollaston's table of chemical equivalents, has been issued by Dr. Paris. According to the common opinion, wheat is placed as a more nutritive substance than the oat, probably from its containing a large proportion of vegeto-animal matter not in the oat. We believe this arrangement, however, is rather contradicted by experience. In Derbyshire the labourers in the mines have declared that they could not perform their severe labour at all, or at least so well, if fed on wheaten instead of oaten bread ; and it has been found by experiment, that a horse, a superior hunter to another horse, became inferior by feeding him with wheat, while by returning to oats he recovered his superiority.

PERPETUAL MOTION.

M. Maillardet of Neufchatel announces, in a foreign journal, that he has succeeded in dissolving the celebrated problem of perpetual motion, so long regarded as a scientific chimera. The piece of mechanism to which he applies his principle is thus described :—It is a wheel, around the circumference of which there

there is a certain number of tubes, which alternately radiate or turn in towards the centre, rendering the moving power at one time strong, at another weak; but preserving throughout such an intensity of force, that it is necessary to keep it in check by a regulator.

ELECTRICAL PHÆNOMENA.

On the evening of the 11th March a very uncommon electrical phænomenon appeared at Belfast, at intervals, between seven and eleven o'clock, nearly due west. Its general shape was that of a comet, but much larger, with its tail upwards; its *nucleus* might be about four or five degrees in diameter, and thirty degrees from the horizon; but although its shape, size, and intensity of light were constantly varying, its position remained nearly the same. It was now and then accompanied by flashes of lightning. The evening was dark, rainy, and stormy.

A letter from Corsica of the 3d April says, "For three months we have had no rain, and the most incessant and terrible winds have prevailed. In the middle of March a dreadful conflagration appeared in the canton of Venaco; in less than three hours the flames had destroyed a surface of more than two leagues of this fine country. Fifteen houses were burnt in the village of Poggio. It is believed that the cause of this fire was electrical, and that fires of a similar kind that have happened in other cantons had the same origin."

LONGITUDE.

Mr. David Christieson, teacher in Montrose, is said to have discovered an easy and exact method, by which the longitude may be ascertained in any part of the world, either by land or sea, by means of a meridian altitude of the sun. It is pointed out by a very simple instrument, constructed on mathematical principles, and does not require those tedious calculations from solar or lunar tables, by which the ordinary method becomes frequently liable to such uncertainty. Neither does it depend on time-keepers, which, though brought to great perfection, cannot be implicitly relied on, especially in long voyages, or where the variations of heat and cold may alter the regular motion of these delicate instruments.

SAFETY LAMP.

M. Van Mons has sent us the gratifying intelligence, that the safety lamp of Davy has completely succeeded in the Netherlands. "Fortified with it," he says, "we can penetrate into the foulest mines. We have even opened depôts of gas, and procured its mixture with the proportion of atmospheric air calculated to produce the most prompt inflammation, and the strongest explosion,

sion, but the gas has never taken fire. We use gauze made of stronger wire than with you, in order to guard against any exterior damage from the awkwardness of the workmen; and to prevent the men from opening the lamp, we have also adopted the expedient of a small padlock, with the key of which the master miner is intrusted. The heating of the gauze cloth, however intense it may be, is not attended with any danger, for iron the most incandescent will not affect gas; nothing but flame will kindle it. Some attempts have been made to light a mine by means of its gas, but I am not as yet acquainted with the result. I should think that such a project must be attended with many obstacles."

LECTURES.

Theatre of Anatomy, Medicine, &c. Blenheim Street, Great Marlborough Street.—The Summer Course of Lectures at this School will begin on Monday, June 2, 1817.

Anatomy, Physiology, and Surgery, by Mr. Brookes daily at Seven in the Morning. Dissections as usual.

Chemistry, Materia Medica, &c. daily at Eight in the Morning; Theory and Practice of Physic at Nine, with Examinations by Dr. Ager.

Three Courses are given every year, each occupying nearly four months. Further particulars may be known from Mr. Brookes, at the Theatre; or from Dr. Ager, 69 Margaret Street, Cavendish Square.

Dr. Clutterbuck will begin his Summer Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Monday, June 2d, at Ten o'clock in the Morning.

Mr. Taunton's Summer Course of Lectures on Anatomy, Physiology, Pathology, and Surgery, will commence at the Theatre of Anatomy, Hatton Garden, on Saturday, May 24th, at Eight o'clock in the Evening *precisely*, and be continued every Tuesday, Thursday, and Saturday, at the same hour.

Mr. Bakewell will deliver a Series of Lessons in Geology, in the Argyle Rooms, during the present month (May), illustrated by numerous original drawings, and by mineral specimens and experiments. In a science which presents new and interesting objects of inquiry at every step, a plan of instruction which admits of questions and explanations respecting the subjects of each lesson, or the specimens, will be found to possess many advantages over the formality of public lectures.

LIST

LIST OF PATENTS FOR NEW INVENTIONS.

To Daniel Wheeler, of Hyde-street, in the parish of St. George Bloomsbury, and county of Middlesex, colouring-maker, for his new or improved method of drying and preparing malt.—6 months allowed for specification, dated 28th March 1817.

To Edward Nicholas, in the parish of Llangattock Vibon Avell, in the county of Monmouth, farmer, for his new plough for the purpose of covering with mould wheat and other grain when sown.—19th April.—2 months.

Astronomical Phænomena, May 1817.

D. H. M.		D. H. M.	
1. 0. 0	☽ in perigee	17.23. 2	☽ ☿
2. 2.14	☽ δ ♍	20.23.53	☉ enters ♊
2. 3.16	☽ β ♍	23. 0. 0	☽ η ♌
2. 5.19	☽ ν ♍	28. 9. 2 I	of a^2 ♎ * 6′ S of
3. 9. 4	☽ d Ophiuchi	28.10. 5 E	☽'s centre.
4.10.15	☽ λ ♐	29. 0. 0	☽ in perigee
7. 7.40	☽ ε ♑	29. 5.40	☽ κ ♎
14. 0. 0	☽ in apogee	29. 9 51	☽ λ ♎
15. 0. 0	☉ eclipsed inv.		

On the 21^d 11^h Venus will be in conjunction with the Sun, the planet 2° 22′ to the north; but his nearest approach will be on the 22d. She may be seen in this situation with a telescope of moderate power, provided an additional tube be attached to the end, to prevent the direct rays of the sun falling on the object-glass.

I have ound a tube of paper, blackened on the inside, of about two feet long, to answer exceedingly well.

I beg to apologize for an error in the occultation of a^2 ♎ in your last Magazine, which arose thus: That occultation is one of the few noticed by the Nautical Almanac: upon comparing my own calculation with it, I found a difference of about an hour; and not doubting but the error must be in mine, I (too confidently it seems) adopted the numbers there given. My own were I 3^d 10^h 17′, F 3^d 10^h 42′, and which, from an observation of the immersion, I believe were not far from the truth. It is probable that this error in the Nautical Ephemeris is a typographical one; but surely, as these errors are as likely to mislead as those of calculation, they require to be as diligently guarded against. I am sorry to notice many errors of the same kind in the present month; for instance, the geocentric longitude of Mars on the 19th and on the 25th appears 10^s, it should be 11^s. The eclipses of Jupiter's first satellite on the 1st and 15th, and that of the second on the 16th, were visible, although they were not marked so. Yours, &c.

* * *

Meteoro-

Meteorological Observations kept at Walthamstow, Essex, from March 13 to April 15, 1817.

[Usually between the Hours of Seven and Nine A.M.]

Date.	Therm.	Barom.	Wind.
March			
13	48	29·90	W.—Cloudy; gray day; bright star-light; *aurora borealis* N. and NE. changing form and place; stars seen through at 11 P.M.; very bright N; at 11½ very dark, and neither stars nor *aurora borealis* visible.
14	44	30·22	Sunshine; very fine warm day; bright star-light.
15	42	30·32	N.NW.—White frost early; hazy; sun through fog; at 9 dark night at 11 P.M. bright star-light.
16	35	30·22	N.—Hazy; clear and clouds; *cumulostratus,* fine day; star-light.
17	34	30·22	NE.E.—Foggy; very fine warm day; star-light. New moon.
18	32	30·23	NE.W.—Hazy and sun; fine sunny day; star-light.
19	47	29·91	W.—Clouds and wind; cold day; sunshine P.M.; star-light; *aurora borealis* at 8½ P.M.
20	32	29·70	W.—Sun and wind; fine cold day; slight showers; bright star-light; at 9 P.M. *aurora borealis* NW.
21	27	29·88	W.—Snowing and sun; sunshine and windy; star and moon light.
22	22	29·90	NS.—White frost and sun; fine day; wind abated; clear star- and moon-light.
23	29	30·00	SE.—White frost, and sun and hazy; very fine day; cloudy.
24	42	29·89	SW.—Cloudy; *cumuli;* some drops of rain; fine day, but clouds; cloudy.
25	45	29·75	W.NW.—Hazy; and slight rain; sun through clouds; showers and sun; *cirrostratus;* fine orange sun-set; star- and moon-light.
26	46	29·93	SW.S.—Hazy and windy; hazy all day; rainy after 4 P.M.; showers and wind; *cirrostratus*; moon and some stars. First quarter.
27	38	30·00	NW.—Sun, and *cirrostratus* and *cumuli;* very fine day; windy; perfectly calm; moon- and star-light.
28	38	30·00	S.—Hazy; rain; showery afternoon; hazy and moon-light.

March

March			
29	50	29·88	S.—Damp; showers and wind; cloudy and windy.
30	45	30·01	SW.—Sun; *stratus* NW. horizon and wind; very fine day; cloudy and windy.
31	44	30·20	NW.—Clear and some *cumuli* and windy; fine day; at 9 P.M. clear; moon and stars; afterwards hazy.
April			
1	45	30·55	NE.SE.—Clear above; *stratus* low; very fine day; clear; moon and star-light. Full moon.
2	42	30·43	S.SE.—Foggy; and sun through clouds; very fine day; clear star-light.
3	40	30·34	SE.—Sun and wind; fine hot day; star-light.
4	40	30·34	NE.—Sunshine; fine day; cooler than yesterday; bright stars and *cumulostratus.*
5	43	30·33	SE..—Gray and cold; fine sunny day; clear star-light.
6	39	30·33	N.—White frost early; fine sunshine; clouds, and some sun; cold day; clouds and windy.
7	40	30·45	E.—*Cirrostratus* and sun; very fine day; star-light.
8	39	30·22	E.—Foggy; very fine day; star-light. Moon last quarter.
9	40	29·98	N.—*Cumuli;* clouds and wind; cold day; dark night at 9 P.M.; ; bright star-light 11 P.M.
10	33	29·98	N.—Sun and wind; snow in the night; snow on the ground; fine day; windy; snow; stars at 3 P.M.; bright star-light.
11	28	30·32	N.—White frost and sunshine; fine day; less windy to-day than for many days past; bright star-light.
12	41	30·10	NW.—Wind and *cirrostratus;* at noon rain, *cumuli* and windy; star-light.
13	47	30·09	NW.—Gray; slight rain from about 9 A.M. to 1 PM.; fine afternoon; star-light.
14	50	30·00	NW.—*Cirrostratus* and windy; slight rain; fine day; star-light; slight *aurora borealis* at 11½ P.M.
15	52	30·00	NW.—Cloudy and windy; floating *cumuli* and sun; fine day; star-light and windy.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1817.	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Mar. 16	28	48·	30·37	Very fine—Rime frost at night
17	new	52·	30·43	Cloudy
18	1	53·	30·24	Very fine
19	2	45·	29.90	Fair—sharp frost at night
20	3	33·	29·93	Ditto—blows hard from W.—snow P.M.—snowed fast at night
21	4	37·	30·05	Very fine—snow lies yet
22	5	41·	30·08	Ditto ditto
23	6	45·	30·11	Ditto ditto—Wind changed to SE—Rain in the evening
24	7	54·	29·93	Fine
25	8	49·5	29·95	Ditto—Rain A.M.
26	9	55·	28·86	Cloudy—slight rain at 2 P.M.
27	10	46·	30·20	Very fine
28	11	45·	29·96	Rain
29	12	57·	29·85	Cloudy—slight rain A.M.
30	13	54·5	29·92	Ditto
31	14	51·	30·50	Fine
Apr. 1	full	58·	30·63	Very fine
2	16	57·5	30·53	Ditto
3	17	54·	30·53	Ditto
4	18	49·	30·55	Ditto
5	19	50·5	30·48	Ditto
6	20	46·5	30·59	Fair—damp, and inclined to rain—rime frost at night
7	21	51·	30·65	Very fine
8	22	58·	30·16	Ditto
9	23	44·5	30·15	Cloudy—frost—snow in the night
10	24	38·	30·18	Ditto—a little snow
11	25	43·5	30·38	Fair—Very sharp frost in morning
12	26	50·	30·13	Slight showers
13	27	54·	30·23	Cloudy
14	28	56·	30·19	Fine

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For April 1817.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
March 27	39	45	36	29·99	36	Fair
28	40	49	45	·70	22	Small rain
29	45	50	47	·96	30	Cloudy
30	50	57	46	·99	45	Fair
31	45	58	45	30·38	47	Fair
April 1	45	60	40	·42	65	Fair
2	41	58	44	·21	56	Fair
3	45	60	45	·23	71	Fair
4	44	56	39	·25	46	Fair
5	40	46	40	·26	39	Fair
6	40	45	40	·27	27	Cloudy
7	44	53	40	·31	42	Fair
8	40	58	45	29·92	57	Fair
9	44	47	38	·90	43	Cloudy
10	33	40	32	·92	52	Fair
11	32	45	40	30·20	50	Fair
12	44	52	45	·08	46	Fair
13	45	54	48	·02	32	Cloudy
14	47	60	51	·01	56	Fair
15	55	62	50	29.99	70	Fair
16	50	50	39	·94	58	Fair
17	40	47	40	30·18	44	Cloudy
18	40	48	42	·35	42	Fair
19	41	56	44	·36	63	Fair
20	44	55	45	·36	66	Fair
21	45	56	44	·34	60	Fair
22	42	54	40	·25	50	Fair
23	40	53	38	·16	46	Fair
24	38	52	42	·19	32	Cloudy
25	40	42	40	·16	29	Cloudy
26	42	47	45	·06	32	Cloudy

N. B. The Barometer's height is taken at one o'clock.

LXXVIII. *Observations on the Solution of Exponential Equations.* *By* G. A. WALKER ARNOTT, *Esq. Edinburgh.*

To Mr. Tilloch.

SIR, — HAVING lately had occasion to enter upon some algebraic problems involving exponential equations, I was very much surprised at the manner of solution used by most writers on the subject; who, instead of finding a direct approximation for the purpose, had recourse to what is commonly called The Rule of Trial and Error, or Double Position. This may be preferable in some, nay in many cases, and then it gives by far the quickest mode; but in others, as in the equation $x^{x^x}=a$, it gives, after one or two trials, an answer by no means so accurate as might be desired. This I ascribe to the confused mixture of parts of logarithms with the numbers themselves.

Thinking, from the mode of solution universally employed, that there might be none other known, or at least none so simple, as that its directness of approximation might compensate for the difficulty of solution, I considered that some general formula might easily be discovered, by attending to the nature of logarithmic series; and I was not disappointed in my researches on this point. It is to the explanation of some of these formulæ that I intend to devote the following pages.

Under exponential equations, we comprehend those which have for the index of one of the sides a variable or unknown quantity. They may all be divided into three classes. Under the first come those defined by the formula $b^x=a$, or where the exponent of the power only is variable. Under the second class come those defined by the formula $x^x=a$, or $\frac{1}{a^x}=a$, which is the same with $x^x=\frac{1}{a}$; in these both the quantity and its index are variable and equal. The third and last class comes under the formula $x=a^x$, where both the quantity and the exponent of its root are variable and equal. Besides these many more might be enumerated, such as $x^y=a$, the general formula of our second class; also a modification of this, $x^{\frac{1}{y}}=a$, or $x=a^y$, the equation to what is called the exponential curve, which is in fact a logarithmic curve, whose subtangent or modulus is $\frac{1}{\mathrm{HL}a}$; and many more. But as these are indeterminate equations, unless y be some *function* of x; and as it is only those capable of one direct answer that I am about to examine, I do not think it necessary to

to comprehend them at all under my division. I may remark besides, that exponential equations are divided into orders, according to the number of exponents above one another; thus x^x, a^x are of the first order, x^{x^x}, a^{x^x}, are of the second order; and so on. This subdivision was, I believe, first noticed by Bernoulli.

As to the first class, or those under the form $b^x=a$, I have nothing new to say, the common mode of solution being quite sufficient. I shall therefore point out how it may be obtained, and take an example in illustration. Let then $b^x=a$; by taking the logarithms of both sides, we have $x\text{L}b=\text{L}a$, or $x=\frac{\text{L}a}{\text{L}b}$, which gives the common rule.

Example. Let $2^x=100$, then $x=\frac{\text{L}100}{\text{L}2}=\frac{2\cdot00000}{\cdot30103}=6\cdot64386$.

This order of the first class is far simpler than any of the other orders or classes, as we shall immediately find. As for the second order of this class, or $b^{x^x}=a$, that is $x^x\text{L}b=\text{L}a$, or $x^x=\frac{\text{L}a}{\text{L}b}$, it is evident that it comes under the first order of the following class.

On account of the second class being as it were the key to the others, I shall explain the rules of approximation rather more minutely than those of the third. We shall also confine our attention solely to the first and second orders, and shall explain them separately.

1st. Of those equations under the form $x^x=a$. These admit of being converted into an infinite series, which, though not a fast converging one, yet deserves a place here on account of its simplicity. It may be derived as follows:

Let $x^x=a$, by taking the logarithms, $x\text{L}x=\text{L}a$, or $x=\frac{\text{L}a}{\text{L}x}$. Consequently $\text{L}x=\text{L}\left(\frac{\text{L}a}{\text{L}x}\right)=$*$\text{L}''a-\text{L}''x$; but instead of this last term, we may substitute the logarithm of the whole of $\text{L}''a-\text{L}''x$, then $\text{L}x=\text{L}'a-\text{L}(\text{L}''a-\text{L}''x)$; and by similar substitutions continually repeated, we get $\text{L}x=\text{L}''a-\text{L}(\text{L}''a-\text{L})(\text{L}''a-\text{L})$ &c. *ad infinitum*. And x is equal to the corresponding number. But x admits of being obtained by an infinite series without logarithms: thus $x^x=a$, or $x=a^{\frac{1}{x}}$, by substitu-

* In this and the following I make use of L'' to denote the logarithm of a logarithm, or the second logarithm; L''' to denote the third logarithm; and so on.

ting

ting instead of the exponent x, its value $a^{\frac{1}{x}}$, and always repeating the operation, we have at length $x=a^{\left(\frac{1}{a}\right)^{\left(\frac{1}{a}\right)^{\left(\frac{1}{a}\right), \&c.}}}$

I will by no means say that these series are easily applicable to practice: far from it, it is almost impossible to use them. We may there reject them as inconvenient; and I shall point out another, which is not attended with such difficulty in the application.

Let $x^x=a$, then $x\mathrm{L}x=\mathrm{L}a$; and let $x=b+y$, where b is the nearest integer or approximate value of x. Hence $x=b\left(1+\frac{y}{b}\right)$, and $\mathrm{L}x=\mathrm{L}\left(b(1+\frac{y}{b})\right)=\mathrm{L}b+\mathrm{L}\overline{1+\frac{y}{b}}=\mathrm{L}b+\mathrm{M}\times\left(\frac{y}{b}-\frac{y^2}{2b^2}+\frac{y^3}{3b^3}-\&c.\right)$ by the nature of logarithmic series, M being the modulus; or, omitting all but the first two terms, as it is only an approximation we desire, and multiplying by $x=b+y$, we have $x\mathrm{L}x=\mathrm{L}a=(b+y)\left(\mathrm{L}b+\frac{\mathrm{M}y}{b}-\frac{\mathrm{M}y^2}{2b^2}\right)=b\mathrm{L}b+y\mathrm{L}b+\mathrm{M}y-\frac{\mathrm{M}y^2}{2b}+\frac{\mathrm{M}y^2}{b}$, omitting any term that involves the cube of y; by arranging and transposing therefore, we have $\frac{\mathrm{M}}{2b}y^2+y(\mathrm{M}+\mathrm{L}b)=\mathrm{L}a-b\mathrm{L}b$; and hence $y^2+2y\left(b+\frac{b\mathrm{L}b}{\mathrm{M}}\right)=2b\left(\frac{\mathrm{L}a}{\mathrm{M}}-\frac{b\mathrm{L}b}{\mathrm{M}}\right)$; or, what is simpler, $y^2+2y(b+b\mathrm{HL}b)=2b(\mathrm{HL}a-b\mathrm{HL}b)$, which quadratic equation being solved, and added to b, gives

$$x=b+y=\sqrt{b\left(\frac{2\mathrm{L}a}{\mathrm{M}}+b+\frac{b\mathrm{L}^2b}{\mathrm{M}^2}\right)}-\frac{b\mathrm{L}b}{\mathrm{M}}, \text{ or simpler thus,}$$

$$x=\sqrt{b(2\mathrm{HL}a+b+b\mathrm{HL}^2b)}-b\mathrm{HL}b.$$

Example. Let $x^x=100$, and therefore $b=3$. Then if we use hyperbolic logarithms,

$x=\sqrt{27{\cdot}63102+9+10{\cdot}86256}-3{\cdot}29584=6{\cdot}8915-3{\cdot}2958=3{\cdot}5957$, or $3{\cdot}59+$. Assuming $b=3{\cdot}59$, we may get a second approximation, and then

$x=\sqrt{33{\cdot}0651220+12{\cdot}8881+21{\cdot}0549339}-4{\cdot}588565=8{\cdot}185853-4{\cdot}588565=3{\cdot}597288$, which is true to the last figure, which should be 5.

As, however, this mode requires the extraction of the square root, and other intricate operations, the following is preferable, as it does not require some of these, and is after the first approximation equally correct with the other. Let us to derive

this

this proceed as above; but instead of two terms of the logarithmic series, only take the first, and reject all the second powers.

Then $b\mathrm{L}b+y\mathrm{L}b+\mathrm{M}y=x\mathrm{L}x=\mathrm{L}a$, or $y(\mathrm{M}+\mathrm{L}b)=\mathrm{L}a-b\mathrm{L}b$, or $y=\frac{\mathrm{L}a-b\mathrm{L}b}{\mathrm{M}+\mathrm{L}b}$.

If we use the hyperbolic logarithms, $x=\frac{b+\mathrm{HL}a}{1+\mathrm{HL}b}$. If the common tabular logarithms, then $x=\frac{b\times\cdot4342945+\mathrm{L}a}{\cdot4342945+\mathrm{L}b}$.

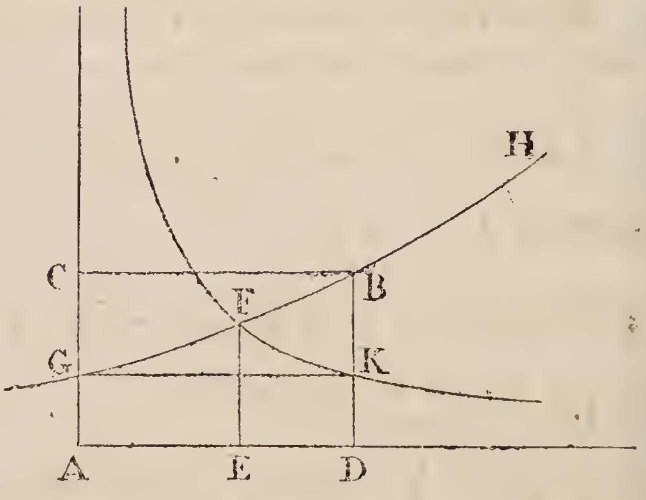
Example. Taking, as above, $x^x=100$, and $b=3{\cdot}59$; then by the hyperbolic logarithms $x=\frac{8{\cdot}1951702}{2{\cdot}2781522}=3{\cdot}597288$, the same as before.

Having therefore seen that by omitting the second and higher powers of the small addition y we have less labour, and nearly as great accuracy, we shall in future not take them into consideration, but employ the first power only.

Frequently we may approximate faster without finding x at first, but only y, and then also it will be of advantage to have x under the form $b-y$, in which case $\mathrm{L}\left(1-\frac{y}{b}\right)=-\frac{\mathrm{M}y}{b}$ nearly; and therefore as in the second formula given above, $y=\frac{b\mathrm{L}b-\mathrm{L}a}{\mathrm{M}+\mathrm{L}b}$. If we wished x this way, then $x=b-y=\frac{\mathrm{M}b+\mathrm{L}a}{\mathrm{M}+\mathrm{L}b}$, the same as found before by another mode.

The more general form, where $y^x=a$ is capable of solution in the same way as the above, when y is a simple function of x, that is, when it becomes the equation $(cx)^x=a$. But as the mode may be easily derived from the former, I shall merely state the result. It is this: if b be an approximate value of x, then $x=\frac{\mathrm{M}b+\mathrm{L}a}{\mathrm{M}+\mathrm{L}bc}$.

Before proceeding to the second order of this class, I may mention a simple geometrical construction of the equation $x^x=a$. Let there be any logarithmic curve GH, whose logarithms begin at the given ordinate AG, which is unit. Produce AG till AC be equal to a; draw CB parallel to the axis to cut GH, and BD parallel to AC; also GK parallel to AD; through K describe an equilateral hyperbola KF, between the asymptotes AC and AD.

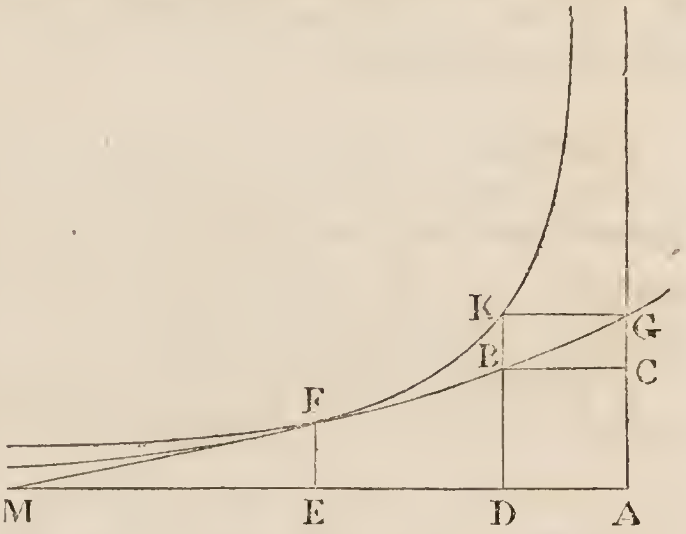


Let it cut the logarithmic curve in F; then FE $=x$.

For

For by construction $BD=a$, and $AD=La$, whence $ADKG=1\times La$; but by the property of the hyperbola $AK=AE\times EF=EF \log. EF=La$, whence $EF^{EF}=a$, and $EF=x$.

This construction might be shortened by making $AD=La$, from a table of logarithms of the same modulus as the above curve. But upon the whole, I would prefer the former method. Were AC less than AG, x and the hyperbolic curve would lie on the opposite side: but AC is then liable to a limitation, which evidently happens when the hyperbola touches the logarithmic curve. The limitation, though it shall be found afterwards by the differential calculus, yet admits of a very simple geometrical investigation. Let F be the point of contact; then by completing the construction $AC=a$, when it is the least possible; draw a tangent MF at F, which must touch both curves at that point, and also ME be the subtangent.



But it is a principle of the hyperbola, that the *double* tangent, or line touching the hyperbola and lying between the two asymptotes, is bisected in F; whence also $ME=EA$; but since ME is a subtangent to the logarithmic curve, and as all these subtangents are equal, if a tangent were applied at G, AE will be the subtangent or modulus of the system. Hence the construction is manifest: for we have only to make AE a subtangent to the logarithmic curve, raise the perpendicular EF, which gives x; through F between the asymptotes MA, AG, describe the hyperbola FK; draw then GK parallel to AM, and KD to AG; then $BD=a$.

2d. With regard to the second order, one mode may be sufficient; I shall therefore choose the simplest:

Let $x^{x^x}=a$, to find x. Since $x^{x^x}=a$, then $x^x Lx=La$, and $xLx+L''x=L''a$. Let $x=b+y=b\left(1+\frac{y}{b}\right)$, then $Lx=Lb+L\left(1+\frac{y}{b}\right)=Lb+\frac{My}{b}$ nearly: and therefore $L''x=L\left(Lb+\frac{My}{b}\right)=L''b+\frac{M^2y}{bLb}$ nearly. Also, $xLx=(b+y)\left(Lb+\frac{My}{b}\right)=bLb+yLb+My$.

Whence from these we get $xLx+L''x=L''a=L''b+bLb+$

y

$y\left(M+Lb+\frac{M^2}{bLb}\right)$, or $y=\frac{L''a-L''b-bLb}{M+Lb+\frac{M^2}{bLb}}$. By adding this to b we shall have $x=b+y=\frac{Mb+\frac{M^2}{Lb}+L''a-L''b}{M+Lb+\frac{M^2}{bLb}}$. If we make use of hyperbolic logarithms, then M and M^2 are each equal to 1. But if we use the common tabular logarithms, $M=\cdot4342945$, and $M^2=\cdot1886117$.

Example. Let $x^{x^x}=100$, and $b=2\cdot2$. Taking the hyperbolic logarithms $x=\frac{2\cdot2+1\cdot2682993+1\cdot5271802-\bar{1}\cdot7623232}{1+\cdot7884574+\cdot5764996}=\frac{5\cdot2331563}{2\cdot3649570}=$ $2\cdot21279+$. True to the last figure, $2\cdot21280$ being rather nearer the true result, but too great.

It is only of these two orders that I intended to speak; but I cannot refrain from pointing out the direct solution of the problem, when the order is infinite. Let then $x^{x^{x^{\&c.}}}=a$; or we may write it thus, $x^{(x^{x^{\&c.}})}=a$. But all within the parenthesis is also equal to a; whence $x^a=a$, and $x=a^{\frac{1}{a}}$. This, though at first sight the most difficult of all, becomes thus the most simple.

We now come to the third class; but have said so much on the last, that it would be superfluous to give more than the two following modes of solution; one by infinite series, and the other by an approximating simple equation.

Let then $x=a^x$. By substituting in place of the exponent x, its value a^x, we have $x=a^{a^x}$, and by continued substitutions of the value of x, we obtain the simple series $a^{a^{a^{\&c.}}}=x$. This, however, is a very useless series: we shall therefore give the other mode, which will be found much more convenient.

Let then $x^{\frac{1}{x}}=a$, and $\frac{Lx}{x}=La$. Let $x=b+y$, then $La=\frac{1}{b+y}\left(Lb+L\overline{1+\frac{y}{b}}\right)=\frac{Lb}{b+y}+\frac{My}{(b+y)b}$ nearly, $=\frac{My+bLb}{b^2+by}$, and $y(M-bLa)=b^2La-bLb$; and thence $y=\frac{b(bLa-Lb)}{M-bLa}$: from which $x=b+y=\frac{b(M-Lb)}{M-bLa}$.

This formula may be more easily deduced by the consideration, that if $x^{\frac{1}{x}}=a$, then $\left(\frac{1}{x}\right)^{\left(\frac{1}{x}\right)}=\frac{1}{a}$: if then $\frac{1}{x}=z$, then $z^z=\frac{1}{a}$

$\frac{1}{c}$, which comes under the form of the first order of the former class; and, therefore, by comparing the two, we have $z = \frac{Mc+L\frac{1}{a}}{M+Lc}$, (where $c = \frac{1}{b}$ = approximate value of z, and consequently b of x). By substitution z is therefore equal to $\frac{M\frac{1}{b}+L\frac{1}{a}}{M+L\frac{1}{b}}$, whence $x = \frac{1}{z} = \frac{M+L\frac{1}{b}}{\frac{M}{b}+L\frac{1}{a}} = \frac{b(M-Lb)}{M-bLa}$.

Example. Let $x^{\frac{1}{x}} = 1\cdot17$. Let $b = 1\cdot2$; then we have $y = \frac{1\cdot2(\cdot1884044-\cdot1823216)}{1-\cdot1884044} = \frac{\cdot0072994}{\cdot8115956} = \cdot00899$, and then $x = 1\cdot20899$, which only errs in the last figure.

It is evident that this class may be easily converted into the second; and thus the following solution of the second order may be, shortly though indirectly, obtained.

Let $x^{\left(\frac{1}{x}\right)^{\left(\frac{1}{x}\right)}} = a$, then $\left(\frac{1}{x}\right)^{\left(\frac{1}{x}\right)^{\left(\frac{1}{x}\right)}} = \frac{1}{a}$. Let $\frac{1}{x} = z$, and $c = \frac{1}{b}$ = an approximate value of z: then by comparing this whit the second order of the first class,

$$z = \frac{Mc+\frac{M^2}{Lc}+L''a-L''c}{M+Lc+\frac{M^2}{cLc}} = \frac{M\frac{1}{b}+\frac{M^2}{L\frac{1}{b}}+L''\frac{1}{a}-L''\frac{1}{b}}{M+L\frac{1}{b}+\frac{bM^2}{L\frac{1}{b}}};$$

consequently $x = \frac{1}{z} = \frac{M-Lb-\frac{bM^2}{Lb}}{\frac{M}{b}-\frac{M^2}{Lb}+L''\frac{1}{a}-L''\frac{1}{b}}$.

When the order is infinite, this class admits of an easy solution.

Thus $x^{x^{x^{\&c.}}} = a$, then $\left(\frac{1}{x}\right)^{\left(\frac{1}{x}\right)^{\left(\frac{1}{x}\right)^{\&c.}}} = \frac{1}{a}$, and $\left(\frac{1}{x}\right)^{\frac{1}{a}} = \frac{1}{a}$, therefore $x = a^a$.

Before finishing this subject, I may mention that several of this species of equations are subject to the laws of *maxima* and *minima*. To illustrate this I shall take an example.

Thus if $x^x = a$, then a or x^x admits of being a minimum. By putting the ratio of the differentials $=0$, then $\frac{x^x dx(1+HLx)}{dx} = 0$, or $HLx = -1. = 0-1 = HL\frac{1}{2\cdot71828}$, and $x = \frac{1}{2\cdot71828} = \cdot36788$;

X 4 a is

a is therefore equal to ·6922. If a be less than this, no solution can take place. This result might be got easier by considering that if a be a minimum, then La is also a minimum; whence xLx is a minimum, and the ratio of the differentials or $\frac{dx(1+\text{HL}x)}{dx}=0$, and HL$x=-1$, and so on.

We may take one more example, which exhibits the limitation in a very striking manner. It is when $x^{\frac{1}{x}}=a$: for then $\frac{dx(1-\text{HL}x)}{x^2dx}=0$, and HL$x=1$. Whence $x=2{\cdot}71828$, and $a=1{\cdot}4447$, when it is a maximum: that is, if a exceeds this number the question is impossible.

I have now closed my remarks on these equations, still, I am convinced, but very imperfectly handled. Many more observations might be added, and indeed the higher orders especially require a far more ample investigation, and at some future period I may perhaps offer a few more remarks upon them. In the mean time I may only observe, that from the short glances I have cast over them, it is my opinion that to obtain these solutions, formulæ of a different kind from those I have as yet employed must be made use of, and which, I have no doubt, may lead the inquirer to some interesting speculations.

April 3, 1817.

To Mr. Tilloch.

Sir,—Since sending you a few days ago some modes of solution regarding exponential equations, another mode (and an extremely simple one) has occurred to me, the insertion of which along with the other part would much oblige, sir,

Yours &c.

Edinburgh, April 20, 1817. G. A. Walker Arnott.

The mode which I now offer is founded on the principle of Taylor's theorem: it is capable of greater extension than the former modes, as we can apply it, with some address, to any order.

Taylor's theorem is this: that if y be a function of z, and z receive an addition c, y then becomes equal to

$y+c\frac{dy}{dz}+\frac{c^2}{1\cdot2}\cdot\frac{d^2y}{dz^2}+\frac{c^3}{1\cdot2\cdot3}\cdot\frac{d^3y}{dz^3}+\&c.$ To apply this theorem to the subject in question, the two first terms of the series are quite sufficient, and we may take an example of the best mode to use it.

1st. Let $x^x=a$, and let b be an approximate value of x, and correspond to y in Taylor's theorem. Let also bLb, which is evidently a *function* of b, correspond to z: then if bLb receive an addition c, so that $b\text{L}b+c=\text{L}a$, or $c=\text{L}a-b\text{L}b$, the two first

first terms of the series are $x=b+c\,\frac{db}{d.(bLb)}=b+\frac{La-bLb}{M+Lb}$, the equation we had before.

2d. Let $x^{x^x}=a$, or $xLx+L''x=L''a$. Then as above $bLb+L''b$ corresponds to z in the theorem. Let this receive an addition c, so as to make it $=L''a$, then $x=b+c\,\frac{db}{d\cdot(bLb+L''b)}=$ $b+\frac{L''a-L''b-bLb}{M+Lb+\frac{M^2}{bLb}}$.

3d. Let $x^{x^{x^x}}=a$, then by the same mode $bLb+L''b+L\,\overline{1+\frac{L''b}{b^bLb}}$ corresponds to z; and then $x=b+c\,\frac{db}{d.\left(bLb+L''b+L\,\overline{1+\frac{L''b}{b^bLb}}\right)}=$

$$b+\frac{L'''a-L''b-bLb-L\,\overline{1+\frac{L''b}{b^bLb}}}{M+Lb+\frac{M^2}{bLb}+\frac{\frac{M^2}{b}-L''b\left(\frac{M}{b}+MLb+L^2b\right)}{LbL''b+b^bL^2b}}.$$

But it is quite needless to pursue this further, as I think I have sufficiently explained the mode for any person to make formulæ for himself.

LXXIX. *Analysis of the Labours of the Royal Academy of Sciences of the Institute of France during the Year* 1816.

PHYSICAL PART.

By M. CUVIER, *Perpetual Secretary.*

Physics and Chemistry.

It is known that different bodies, and especially different liquids, are dilated by heat in very different proportions.

M. Gay Lussac has endeavoured to discover some law which might indicate the regulation of these proportions. For this purpose, in place of comparing the dilatations of different liquids above and below one uniform temperature, he sets out from a point, variable as to temperature but uniform as to the cohesion of particles; from a point where every liquid rises into ebullition under a given pressure. And among those which he has tried, he has found two which, setting out from this point, become equally dilated;—these are alcohol and sulphuret of carbon, which boil, the former at 78°41, the second at 46°60. Examining then into the analogies of these two liquids, M. Gay Lussac has discovered that they resemble in this point; that the same volume of

of each at its boiling temperature gives, under an equal pressure, the same volume of vapour; or in other terms, that the densities of their vapours bear the same relation as those of liquids to their respective boiling temperatures.

M. Gay Lussac intends to pursue his experiments, and to present very soon more complete researches upon the dilatation of liquids and upon their capacity for caloric.

Among the delicate questions which are at present agitated in chemistry, one of the most important respects the proportions in which the elements unite in forming different combinations. These sorts of researches are subject to great difficulties, because it is not always possible to obtain insulated combinations of gas; and when it is wished to extract them from the salts of which they make part, they are liable to be decomposed or altered by the intermixture of other principles of these salts, or of water which almost always presents itself. It is thus that the remarkable differences between the results of Davy, Dalton, and Gay Lussac, respecting the combinations of azote and oxygen, may be explained.

From experiments presented this year to the Academy by M. Gay Lussac, it appears that nitrous gas contains a volume of azote and an equal volume of oxygen without condensation; that in certain circumstances it presents a combination of a volume of azote with a volume and a half of oxygen; and to this M. Gay Lussac has given the name of pernitrous acid (*acide pernitreux*):—That ordinary nitrous acid is composed of a volume of azote with two volumes of oxygen:—lastly, That there is in nitric acid a volume of azote and two volumes and a half of oxygen.

Among these different varieties of oxides or acids which have azote for their base, he has found one which is obtained from the distillation of the neuter nitrate of lead previously dried. It is a very volatile liquid of an orange colour. M. Gay Lussac regards it as nitrous acid, the elements of which are retained by the action of water which he supposes makes part of it. But M. Dulong has satisfied himself by very exact analyses that it does not contain water; and he calls it for this reason anhydro-nitrous acid (*acide nitreux anhydre*). The result obtained by M. Dulong has been confirmed synthetically. A volume of nitrous gas, and somewhat more than two volumes of oxygen, submitted to an artificial cold of twenty degrees, give that acid, which among other properties changes colour not only by intermixture with water, but by heat;—colourless at 20° below zero, it becomes of an orange hue at 15° above it, and almost red at 28°. Four parts of nitrous gas and one part of oxygen gas, condensed by cold, have yielded a much more volatile liquid than the

the preceding, of a deep green; which M. Dulong regards as a simple mixture of nitrous acid, and another acid in which the proportion of nitrous gas is much greater.

M. Dulong has also investigated the proportions in which oxygen combines with phosphorus in the formation of acids. Hitherto only two acids have been recognised, but his researches have led him to think that four exist.

The first in which the least of oxygen enters is obtained by throwing in water an alkaline phosphuret. The phosphuretted hydrogen is disengaged, and the oxygen of the water forms with the remaining phosphorus an acid which continues combined with the alkali, but which is expelled by sulphuric acid. M. Dulong terms it *hypophosphorus*, but he believes that its radical is in part composed of hydrogen.

A second acid, to which M. Dulong transfers the name of phosphorous, is obtained by decomposing water by a combination of chlorine and a minimum of phosphorus, a decomposition from which two acids result; viz. hydrochloric or muriatic acid, and this which M. Dulong calls phosphorous acid. M. Dulong states it to be composed of 100 parts of phosphorus and nearly 75 of oxygen.

The third acid is that which is produced by the slow combustion of phosphorus in air. It decomposes when saturated with phosphoric acid and with phosphorous acid, and gives at once phosphites which are more soluble, and phosphates which are less so. M. Dulong does not regard it as a simple mixture, but rather as a combination of these two acids, somewhat resembling saline combinations, and in which the phosphorous acid serves as a base. According to that opinion, he proposes to name it *phosphatic*, in order to recall the analogy which it bears to the phosphates.

The last term of oxygenation is *phosphoric* acid: the proportion of phosphorus to oxygen is here 100 to 124. It is obtained by the quick combustion of phosphorus, or from the decomposition of water by the chloruret (*chlorure*) of phosphorus, and in many other ways. It is identically the same with that which is extracted from the bones of animals.

Three Dutch chemists, Messrs. Van Marum, Deyman, and Paets Van Troostwick, made known in 1796 a gas composed of hydrogen and carbon, which they named *olefiant* gas, on account of the singular property it possessed of forming an oily liquid by its mixture with oxygenated muriatic gas. According to the theory which then prevailed respecting oxygenated muriatic gas, it might have been believed that the oxygen united itself to the carbonetted hydrogen, and produced thus a sort of oil: but now that we have come to regard this gas as a simple body, to which

Sir

Sir H. Davy has given the name of chlorine, another explanation must be sought for. Messrs. Robiquet and Colin have devoted themselves to this object. They have found that by passing slowly into a balloon a volume of olefiant gas and two volumes of chlorine, they become entirely converted without any residue into an oily liquid, which decomposed by heat gives hydrogen not saturated with carbon, a deposit of carbon, and a good deal of muriatic gas—that is to say, according to the new theory, hydrochloric gas. The chlorine therefore enters in susbtance into the oily liquid. But is it there as chlorine, and united directly to the subcarburetted hydrogen? or, rather, is it there united to the hydrogen, and as hydrochloric acid, or in other words muriatic? It is to the first of these conclusions that Messrs. Robiquet and Colin have been led, by indications deduced from the specific weight of the components and of the compound; while muriatic ether, which has many resemblances to this oily liquid, appears to them on the contrary to be formed by the union of hydrochloric gas with carburetted hydrogen.

M. Chevreul continues still to labour with the same zeal in his Chemical History of Greasy Bodies. In a memoir presented to the Academy this year, this laborious chemist has begun to examine the causes to which the consistency, the odour and the particular colour of some oils and fats are owing. The varieties of consistency are regulated by two general principles of greasy bodies; but the other differences depend on particular and foreign principles. M. Chevreul proposes a system of nomenclature analogous to the rest of chemical nomenclature, as well for the principles which he has discovered as for their saline combinations. The two principles of grease ought accordingly to be named *steatine* and *elaine*, from the Greek words which signify fat and oil. Its principal acid, that which is most consistent, will be *margaric* acid; the other, *elaic* acid. Spermaceti will take the name of *cetine*, &c. &c. These names are no doubt burdensome to the memory, but that is an inconvenience inseparable from the progress of science.

Mineralogy and Geology.

Greenland has for several years furnished a stone in small crystals of a sea-green colour, which has been named *sodalite* from its containing nearly a fourth of its weight of soda united with silex and alumine.

The Count Dunin-Borkowsky, a Gallician gentleman and mineralogist, as zealous as well informed, has discovered a colourless variety of the same stone in large prisms, in that part of Vesuvius called *Fosso-grande*, so celebrated for the number and variety of the minerals with which it has supplied collectors.

The

The composition of this species of the stone, from its great analogy to that of glass, might be supposed to be of volcanic origin; if it were not accompanied with a multitude of other kinds which have nothing in common with glass; and if the sodalites of Greenland were not found in places where there is no trace of subterranean fires.

The object of geology, in the scientific form which it has attained in modern times, is not so much to imagine, as formerly, systems upon the states through which the globe has passed, as to describe exactly its existing state, and the relative position of the masses which compose its exterior.

In this last respect these masses have, it is well known, been distinguished into primitive,—that is, masses in which there are no traces of organized bodies, and which are supposed anterior to life,—and into secondary, which are all more or less filled with organic remains, and which must of course have been formed since these existed. These masses are besides generally different both in point of form and of the materials of which they are composed. It has been even for a long time believed that these materials are placed in a succession of order equally determined; so that no such masses as were deposited before the existence of organic bodies can have been deposited since, and *vice versâ*.

More correct observations have since shown this idea, as to the depositation of the strata of the earth, to be quite erroneous. It has been observed, that between these two sorts of masses there exist mixtures where the ancient or primitive strata have been reproduced after new or secondary formations; that is to say, some organized bodies which are covered by masses of the same nature as those which were supposed to have ceased depositing themselves since life appeared on the globe. These memorials of a change from one state of things to another have been called transition earths.

It is not, however, always easy to recognise them as such; and M. Brochant, in a memoir which he recently published, had need of all his sagacity to attach to this intermediate class the greater part of the valley of Tarentaise; in as much as he had not then discovered some shells, the existence of which in these rocks, as since ascertained, has confirmed in a most flattering manner the conjectures and the reasonings of that learned geologist.

M. Brochant has since extended his researches on the same subject, and directed them chiefly this year to the ancient gypsums which are found in such abundance in certain parts of the Alps, and of whose enormous masses all travellers who have crossed Mont-Cenis have taken notice. After having described with a scrupulous exactness all the circumstances of their position, and having often traversed the mountains, on the shelves

of

of which they present themselves, the author compares them in point of composition and situation with transition rocks, and proves that they ought not to be ranked in that class.

The primitive masses themselves are not always easy to characterize: the irregularity of their position, the vastness of the spaces through which it is sometimes necessary to trace their relations, and the confounding varieties in their composition, present great difficulties. M. Brochant has furnished us with a striking instance of the mistakes which may be thus occasioned. After long journeys and laborious examinations, he has discovered that the lofty summits of the Alps, from Mount-Cenis even to St. Gothard, and particularly Mont-Blanc, do not consist, as has been believed, of granite properly so called; but of a variety more crystalline, and more abundant of a talcous and feldsparic rock, which often contains beds of metallic minerals. He has satisfied himself at the same time that real granite pervades the southern border of the chain; and, reasoning from analogy, he considers it very probable that the granitic stratum supports the talcous; whence he concludes that the higher summits of the Alps are not relatively the most ancient part of these mountains.

It ought always to be remarked, that the primordiality of granite among known rocks is subject to exceptions. M. de Buch has ascertained, in Norway, that granites evidently recognisable as such are deposited above strata believed to be of a more modern class, and even strata containing petrifactions. The same thing has been observed in Saxony, and even in Caucasus.

M. de Bonnard—who has had the honour of giving to geology the first complete description of the *Ertzgeburge*—of that province of Saxony which is in a manner the country of geology—has endeavoured in that work to determine those places where granite is inferior, and those where it is superior to other strata. It cannot be doubted, after the researches he has made, that the granite of Dohna cannot be in this last situation, as the Saxon geologists have announced; but in other places, and especially near Freyberg, there is every reason to believe in the superiority of the granite. It further appears from his observations, that the chain which separates Saxony from Bohemia is distinguished by also having granite on one side, and that the southern.

The work of M. de Bonnard contains many other valuable details upon the nature and position of the strata of this celebrated province, and also on the rich metallic veins which everywhere traverse it, and on which the industry of the people has been so long exercised. In these respects it is equally interesting to geology and to the art of mining.

M. Heron de Villefosse has also rendered to the same art a very

very great service, by his work on Mineral Wealth. The first volume, which related to the administration of the mines, printed in 1810, has been a long time known and appreciated. The second, in which he treats of the working of them, has been presented in manuscript to the Academy. The author has here added to all the theoretical directions which science affords, a great quantity of practical facts collected by him in his travels, and in the exercise of his functions; so that every precept is in a manner supported by a real example. A magnificent atlas which accompanies the work presents all that can be exhibited of these examples to the eye.

The discovery so important in geology, made by MM. Brongniart and Cuvier, of certain stony beds which contain only land and fresh-water shells, and which cannot of course have been formed by the sea like other shelly beds,—has led to a number of researches in all parts of Europe. M. Beudant, professor at Marseilles, has this year considered this matter in a new light. As in some places fresh-water shells have been found mingled with marine shells, he has endeavoured to ascertain by experiment, how long the molluscæ of fresh water may be habituated to live in salt water, and *vice versâ* with respect to marine molluscæ. He has found that all these animals die quickly when the change is sudden and entire; but when the saltness of the water is gradually increased for the one, and gradually diminished for the other, they become for the most part habituated to live in a water which is not natural to them. Some species, however, resist all conciliation, and cannot suffer any variation in the water which they inhabit.

M. Marcel de Serres has communicated the sequel of his first researches on fresh-water shells, of which an account was given in the analysis of the Academy for 1813. The principal fact which he has made known this year, is a formation of this sort, which he regards as newer than any other, and which he has discovered in seven different places in the neighbourhood of Montpelier. His observations are somewhat similar to those of M. Beudant. He distinguishes the species in the environs of Montpelier, into those which do not appear to be able to live except in fresh water; those which can subsist in briny waters not exceeding a maximum of 2·75; and lastly, those to which marine waters appear necessary. He has by this means explained some very singular mixtures of these organic remains.

The stratum which he describes is in a manner composed of two layers inclosing different shells. The superior one contains both land and aquatic shells. The new formation appears principally on the higher parts of the hills or ridges. A great many land shells

shells and impressions of vegetables have been found there, perfectly similar to the species which exist on the superincumbent soil at present.

While in Europe the principles of geological observation are thus evolved, some zealous naturalists have been applying them to countries more remote, and have found Nature ever faithful to the same laws.

The great labours of M. Humboldt on the structure and elevation of the respective mountains of the two Americas are well known. This learned traveller has presented a prelude to a work which promises to be not less important, in a table of results obtained in India, on the height of different peaks of that immense chain known to the ancients by the name of *Imaus*, and to which the Indians have ascribed the principal facts of their mythology.

According to the trigonometrical measurements of Mr. Webb, an English engineer, four of these peaks are more elevated than Chimborazo; and one of them, the highest mountain known at present on the globe, is 4013 toises or 7821 metres; and even according to other calculations 4201 toises or 8187 metres.

M. Humboldt makes in his memoir a happy use of the laws of vegetable geography, in order to supply the heights of certain ridges which it has not yet been found practicable to ascertain by actual measurement. When a particular plant grows in any place of a ridge, he determines by the latitude what height the ridge cannot exceed.

Within a space more limited M. Moreau de Jonnès, correspondent of the Academy, has also made some very useful observations. He has presented to the Academy a geological chart of a part of Martinique, on which are marked with great care the heights of the mountains and hills, and principally of an extinct volcano which appears to have given birth to these inequalities.

The author has extended his researches to a great part of the Antilles. The elevated centres of these islands consist of volcanic peaks which are named *mornes;* the tufts of lava with which they are overrun are called *barres;* and the denomination of *plainiers* is used to designate the shelves (*plateaux*) which they have formed in their descent.

The islands where there is only one peak and one system of eruption, such as Saba, Neva, St. Vincent, are the smallest, and in an agricultural respect the least important. They have no good ports, because such ports are nothing but the extremities of the valleys left between two or more systems; such as we see in Guadaloupe, Martinique, Dominica, St. Lucia, Grenada, &c. Martinique in particular appears to owe its origin to six volcanic

fires

fires, and shows still six peaks to which it may be wholly attributed. It is the exact topography and mineralogy of one of these six, that of Mont Pelée, which M. de Jonnes has given us. He believes that volcanic nature so general, that he supposes it serves even as a base to those of the Antilles, which present nothing but a calcareous exterior manifestly shelly, such as Barbadoes and the high land of Guadaloupe. Guadaloupe properly so called is formed of four systems of eruption, one of which, the *Soufrière*, still maintains some degree of activity. M. de Jonnes has also given a very accurate description of it in a general statistical view of the island.

Botany.

One of the most important branches of botany, and which more than any other connects it with the great body of physical sciences, is vegetable geography, or the laws of the distribution of plants according to the height of the pole, the elevation of the soil, the temperature, and degree of humidity or dryness of the climate.

M. Humboldt, whose travels have contributed so much to the progress of this as well as other branches of knowledge, has given nearly a complete treatise on it under the title of *Prolegomena de Distributione geographica Plantarum secundum Cœli Temperiem et Altitudinem Montium.* In this work he presents some profound researches on the distribution of heat, both relatively to the positions of places and to the seasons of the year; for not only the lines under which the same mean annual degree of heat prevails are far from being parallel to the equator, but places which have upon the whole an equal mean heat have their summers and winters by no means alike. All these differences must, it is conceived, have a strong influence on the propagation of plants. The author afterwards proceeds to the differences which result from elevation, and which are not regulated by the same laws in all places: and he at last arrives at a consideration altogether new,—that of the laws of the distribution of vegetable forms. By comparing in each country the number of plants of certain families well determined, with the total number of vegetables, he has found a striking regularity of numerical coincidence. Certain sorts become more common in proportion as we advance towards the pole; others, on the contrary, increase towards the equator; while some attain their maximum in the temperate zone, and are equally diminished by too much heat and too much cold. And, which is very remarkable, this distribution remains the same all over the globe, by following not the geographic parallels, but what M. Humboldt calls *isothermal* parallels, that is to say, lines of the same mean heat. So constant

are

are these laws, that whenever we know the number in any country of the species of one of those families of which M. Humboldt has given a table, we may deduce from it a pretty exact calculation both of the total number of vegetables and of the number of species in each of the other families.

The *Prolegomena* to which we allude are placed at the head of the great work which M. Humboldt is at present publishing with Messrs. Bonpland and Kunth, upon the new plants which he has discovered in equinoctial America. This addition, the richest and most brilliant perhaps which botany has received at one time, will appear in six volumes quarto, and contain six hundred plates and the description of more than four thousand species. The first volume, including all the *monocotyledones*, has been published this year: it presents us with thirty-three new genera, and among the *palmæ* alone twenty-three new species. Messrs. Humboldt and Bonpland have published at the same time the conclusion of their description of the *Melastoma*, a work of most magnificent exterior, but which could not be imitated throughout the whole range of vegetables, without incurring an expense and delay as prejudicial to science as to its cultivators.

M. de Beauvois, whose perseverance is also deserving of every praise, has published this year the fourteenth and fifteenth parts of his *Flore d'Oware et de Benin*; and, not satisfied with his ancient collections, he has taken advantage of the extraordinary humidity of this year to pursue his investigation into the class of *Fungi*. The continual rains have so developed this class, that he has discovered many species which have escaped preceding botanists. Such are—a variety of *sclerotium* which has lessened nearly by two-thirds the crop of kidney beans, upon which it propagates itself;—a new species of *spheria*, which has been very destructive to onions; also a new species of *cereda*, which has been still more pernicious to them; and lastly, which is very remarkable, a new genus of parasitic plant which grows upon other parasites, and injures very considerably the vegetables which are obliged to nourish both.

The family of the *dipsacei*, such as the scabious, are, it is well known, very nearly allied to *composites* by many of the characters of their flowers and their fruits;—the most apparent mark which distinguishes them is, that the anthers are entirely free. Botanists have discovered some plants whose flowers are equally formed of many smaller flowers, the anthers of which are united at the bottom only. It has been found doubtful what place to assign them. M. Henri de Cassini, who has examined them at the close of his great work on the class of *Syngenesia* or *Compositæ*, has found that they differ from these—because their anthers are not united at the top, because their style and stigma

are

are of a different conformation, and because the seed is suspended at the top of the cavity of the oveary. From the *dypsaci*, again, they differ in having their anthers united at the bottom, and in the alternation of their leaves. Most of their other characters, however, they possess in common with these two classes. M. de Cassini has hence been led to think that a distinct family might be formed, which would serve in place of two others; and this he has designated by the name of *Boopidées*. These will comprehend the genera *calycera de cavanilles, boopis* and *lacicarpha* of M. de Jussieu.

We announced last year the opinion of M. Decandolle upon that pernicious substance called *ergot*, which shows itself in the ears of rye and some other grains, especially in wet seasons. In the course of the year 1816, M. Virey has made some researches, which lead him to regard ergot as a degenerescence of grain, and not as a fungus of the genus *sclerotium*, as M. Decandolle believes. He says he has observed ergotized grains which had not only preserved their natural form, but in which one might still see remains of stigmas; and he cites the assertion of M. Tessier, that many ears of grain have been observed which were only half-ergotized, and that equally towards the top as towards the bottom.

M. Vauquelin has made on this occasion a comparative analysis of sound rye, of the ergot of rye, and of a *sclerotium* perfectly recognised as such. In the ergot he found neither starch nor gluten in their natural state, but a mucous substance and a vegeto-animal matter, abundant and inclining to putrefaction. It contains a fixed oil quite developed. The principles of sclerotium are altogether different. Without being decisive, these experiments have led some persons to doubt with M. Virey whether ergot be of the tribe of fungi.

Zoology, Anatomy, and Animal Physiology.

Animals have also their geography: for Nature in like manner retains every species within certain limits, by lines more or less analogous to those which restrict the extension of vegetables.

Zimmermann has already given, upon the distribution of quadrupeds, a work which is of some celebrity.

Latreille has since published one upon that of insects. Differences of from ten to twelve degrees of latitude produce always at an equal height particular insects; and when the difference reaches to from twenty to twenty-four degrees, nearly all the insects are different. Analogous changes have been observed corresponding to the longitude, but at distances much more considerable.

The old and the new worlds have kinds of insects peculiar to

each;

each; and the species even of those which are common to both, present very perceptible differences. The insects of the countries which surround the Mediterranean, those of the Black and Caspian Seas, and those also of a great part of Africa, present a great similarity. These countries form particularly the region of the *coleoptera*. America presents us, besides the genera peculiar to itself, with a great number of herbivorous insects, such as the *chrysomela*, the *cassida*, the *papilio*, &c. Those of Asia beyond the Indus possess a great affinity in respect of the families and genera of which they make a part. The species of New Holland, although neighbours to those of the Moluccas, are distinguished by some essential characteristics. The isles of the South Sea and of South America seem to indicate in this respect some general affinities, although the entomology of Africa is directly contrasted in many points with that of South America.

In the west of Europe the prevalence of meridional insects becomes very manifest, when, going from north to south, we arrive at those countries which are favourable to the culture of the olive. The presence of the scorpion announces this remarkable change of temperature; but in North America the same change does not take place until about five or six degrees of latitude nearer the equator. The form of the new continent and the nature of its soil and climate are the causes of this difference.

M. Latreille presents us in the sequel with a new division of the earth by climates. Greenland, although very close to America, appears, according to Otho Fabricius, rather to ally itself in this respect to the north and west of Europe. It may still, however, be considered as an intermedial land between the two worlds. Adopting this view, M. Latreille takes it as the point of departure of a first meridian, which, passing 34° to the west of that of Paris, extends into the Atlantic Ocean, and terminates at the Sandwich Islands in 60° south lat., being the *ne plus ultra* of our discoveries towards the antarctic pole. This meridian, from 84° north lat., the last approximative boundary of vegetation, to 64° south lat., is divided at every twelve degrees by circles parallel to the equator. The intervals form so many climates, which M. Latreille designates by the names of polar, subpolar, superior, intermedial, surtropical, tropical, and equatorial. M. Latreille next divides the two hemispheres by another meridian, which he fixes at 180° to the east of that of Paris, and then separates each continent into two great parts by means of two other meridians; the one 62° more easterly than that of Paris, and touching on the western limits of the Indian Sea; the other intersecting America at 160° to the west of the meridian of Paris, and detaching the part of that continent which geographically, and also perhaps

perhaps in point of natural productions, is more connected with Asia. The two hemispheres are thus divided longitudinally into two zones, eastern and western.

The attention of all Paris has been recently directed to a woman from the Cape of Good Hope, who has been exhibited to the public under the name of the Hottentot Venus. She belonged to a nation in the interior of Africa celebrated among the colonists of the Cape for their ferocity. The shortness of their stature, and especially the enormous projection of the buttocks in the woman, seem to mark them out as a race quite distinct from the Negroes and Caffres by whom they are surrounded. Much has been also said of the *aprons* of these females. By earlier travellers they have been represented very inaccurately, and more recent ones have ventured even to deny their existence.

The individual alluded to having died at Paris, M. Cuvier had occasion to dissect her, and to establish the particularities of her organization. She possessed the apron: but this was neither an overfolding of the skin of the belly, nor a particular organ; but a considerable protrusion of the superior part of the nymphæ, which fell before the opening of the matrice and covered it entirely. The prominences of the buttocks consisted of a cellular tissue filled with fat similar to the bumps of the camel and dromedary. The head displayed a singular mixture of the characteristics of the Negro and the Calmuck; and the bones of the arms, remarkable for their slenderness, presented some remote affinities to those of certain species of the monkey tribe.

One of the most formidable of the venomous tribe next to the rattle-snake is the yellow viper, or *fer-de-lance* of Martinique and St. Lucia, upon which M. Moreau de Jonnes has communicated a very interesting memoir to the Academy. Naturalists place it at present among the genus *trigonocephali*. It abounds in all the principal colonies of France. Some pretend that it was originally transported there out of hatred to the Carribs, by a people on the borders of the Oroonoko; a tradition which may perhaps explain why it remains a stranger to the rest of the Antilles. From the borders of the sea to the summit of the mountains the inhabitants are exposed to its attacks; but its principal retreat is in the fields of sugar-cane, where the great abundance of rats affords it a plentiful subsistence, and where it propagates with an increase proportioned to the number of its young, of which it has generally from fifteen to sixteen at a birth. It sometimes exceeds six feet in length. Attempts have been made, but in vain, to destroy these vipers by means of terrier dogs, of English breed. M. Jonnes proposes to employ against them the bird of prey called *messager* or *secretaire* (*falco serpentarius* of Linnæus), which is so celebrated for devouring ser-

 pents

pents in the environs of the Cape of Good Hope; and government have already taken into consideration the means of transporting that useful species of animal to Martinique*.

M. Cuvier has finished by a memoir *Sur le Poulpe, le Seiche, et le Calmar*, the work which he undertook a long time since upon the anatomy of the *molluscæ*. The genera which have been now mentioned are the most remarkable in this numerous class of animals for complication and singularity of structure. Provided with three hearts, with a nervous system very developed, with large eyes as well formed as those of any vertebral animal, with excretory viscera formed on a plan of which Nature offers no other example—they merit every attention from naturalists.

In making these anatomical researches M. Cuvier has had occasion to discover the nature of a fossil common enough in calcareous beds, and which has hitherto been quite an ænigma to geologists. It is a bony fragment, concave on one side and convex on the other, with a border radiated, and a ridge (*épine*) between the convexity and the border. It is now demonstrated that this is the inferior extremity of a bone of the *seiche* (*Sepia* L.); and if there is any thing to be astonished at, it is, that a resemblance so evident has not been sooner discovered.

The fresh waters of some cantons in the south of France produce a very small shell-fish, similar to a buckler surmounted with a goad pointed and curved. It has been generally believed to be an univalve, and has been called *l'ancyle épine de rose*; but M. Marcel de Serres has ascertained that it is one of the valves of a regular bivalvular shell, the hinge of which has certain peculiar characteristics. He has in consequence formed a new genus of it, which he has named *acanthis*. The animal of this shell has not yet been observed.

The animals without vertebræ, considered in general in regard to the classification and enumeration of the species, form the subject of a large work, of which M. de Lamarck has published the three first volumes, commencing with the microscopic animals, and extending to the tribe of insects. In treating of the *molluscæ*, he introduces a new class which he calls *tuniciers*, formed of those composite *molluscæ* of which M. Savigny has given so singular a history, as also of those simple *molluscæ* which are analogous to those which enter into the formation of the composites.

To the history of corals a valuable addition has been made by a work of M. Lamouroux, on those species the solid part of which is flexible.

One of the most interesting questions in physiology regards the

* See some further notice of the communication of M. Jonnes, in Phil. Mag. for February last.

the origin of the azote, which makes an essential element of animal bodies. M. Majendie has endeavoured to elucidate it by a variety of experiments, which afford many important dietetic indications, and cannot fail to be of considerable use to medical science *.

M. Majendie has also made, in common with M. Chevreul, some experiments to determine the nature of the gas which develops itself at the moment of digestion in the different parts of the elementary canal. In four criminals who had taken, a little before their execution, certain fixed viands, the stomach presented oxygen, carbonic acid, pure hydrogen and azote,—the small intestine the three latter gases but no oxygen;—in the great intestine there appeared, in conjunction with the carbonic acid and azote, carburetted and sulphuretted hydrogen gases. The two last pertained therefore only to the great intestines: oxygen is found only in the stomach; azote and carbonic acid exist throughout the whole canal, and the quantity of the last increases as the aliment descends.

Medicine and Surgery.

Ignorance in medicine is never more dangerous than, when called in to enlighten justice, it leads it astray by inconsiderate assumptions, which draw down upon innocence the shame and the punishment reserved for guilt. A work which M. Chaussier has published on Legal Medicine—the object of which is to concentrate the lights afforded by anatomy, chemistry, and physiology, for determining the causes of death—is on this account of a truly social interest. To the general rules which he prescribes, he adds, as examples, many judicial reports of remarkable cases; with his own remarks on the omissions, the errors, the obscurities, the fallacies which are too often to be met with in these important documents. The whole of this part answers completely to the motto of the work—"*Sontibus inde tremor, civibus inde salus.*"

The author, however, has not limited himself to what is promised in his title. He has also directed his attention to many faults in the ordinary manner of opening dead bodies—faults which have often led to false conclusions on the nature and the seat of diseases. And physiology generally will be much benefited by the great number of acute remarks on functions too little attended to, which this learned physiologist has interspersed through his work.

M. Larrey is one of those few surgeons who have exercised their art in spheres of great extent and variety. Attached to

* For notice of these see Phil. Mag. for February 1817.

the French armies during twenty-five campaigns, he has followed them into the four quarters of the globe, and has had the chief direction of the surgical practice in Egypt and in Russia, as well as in all the intermediate climates,—during epochs of victory the most brilliant and prosperous, as well as in defeats the most calamitous,—in situations of deprivation the most absolute. No variety of occasion has he wanted, and he has profited by them all. To those results of his experience which he has already recorded in various public works, he has added this year some important observations upon the effects of foreign bodies introduced into the lungs, and the operations employed to remove them. When the collection of pus or of blood has forced the lungs to contract, the expulsion of these matters occasions a void in the thorax, which Nature endeavours to fill up either by the production of new matter, or by detaching matter from the sides and other neighbouring parts. M. Larrey has been able to exhibit these changes by the dissection of individuals who, after being cured, had fallen victims to other fatalities. He has also been the first to fix the opinion of practicians, as to the possibility of the amputation of the thigh at its superior articulation, by presenting an actual case of such amputation followed with perfect recovery, and making known the mode by which it may be performed with safety.

Rural Œconomy.

The fur of the beaver, so necessary in the manufacture of hats, becoming daily more scarce and costly, many other furs have been tried, without any being found which could supply its place. M. Guichardiere, hatter in Paris, has employed, with success, as a substitute the fur of the sea otter, and also that of the native otter. It is true that hats wholly composed of such a material would be dearer even than those made of beaver; but considerable profit will be found by overlaying, or as the hatters call it gilding, hats with this fur, the bodies of which are formed of more common stuff:—in the same manner, in short, as has already been long the practice with beaver.

It still remains to us to rank among the useful works which have occupied the members or the correspondents of the Academy of Sciences during the year 1816, the instruction of M. Huzard on the means proper for disinfecting cow-houses, and preserving cattle from the epizooty;—many articles of agriculture inserted by M. Yvart in the New Dictionary of Natural History, particularly one on the breeding of domestic animals; with the History of French Agriculture, by M. Rougier de la Bergerie.

MATHE-

MATHEMATICAL PART.

By M. Le Chevalier Delambre, *Perpetual Secretary.*

"Never, perhaps," says the learned analyzer of this department, "was the zeal of geometricians better sustained—never have they devoted themselves with more constancy to their accustomed labours, to the development of their first ideas, to the completion of works already published; and yet never have we found greater difficulty in giving the history of this branch of the annual labours of the Academy." After a statement of some obstacles arising from the private arrangements of the Academy, "the more," it is added, "mathematics have advanced, the more difficult their ulterior progress has become, the more impracticable it is found to render sensible and striking the results newly obtained." It will not therefore, it is hoped, excite surprise, that in the present analysis little more than the titles of some valuable works should be indicated, nor (if we may be allowed to supply the parallel implied, but not expressed, by the learned secretary) that in entering more at large into other works, a successive exposition of the contents of each should be preferred to any attempt to give a condensed and systematic view of the whole.

The length, however, to which on these accounts the analysis of this branch of the labours of the Academy extends (eighty pages quarto), is a disadvantage which, we regret, precludes the practicability of transferring it to our pages, as we have done that of the preceding department. Not a few of the articles indeed, particularly those of M. Laplace, Mr. Farey, &c. have already been fully noticed in the Philosophical Magazine; and opportunities will afterwards be taken to make our readers acquainted from time to time with such others as merit, but have not yet been under, their observation.

The article of Mr. Farey's to which we have just alluded, and to which the ingenious secretary refers with no more than the respect which it merits, appeared originally in the Philosophical Magazine, vol. xlvii. p. 385, and refers to a curious property of vulgar fractions, which Mr. Farey was the first to discover and communicate to the world *.

* My ingenious correspondent Mr. Farey, referring to this subject in a letter which I lately received from him, writes as follows:

"I still entertain hopes, that some English Mathematician will turn his attention to the demonstration of the properties of *complete fractional Series:* your Correspondent S. A. in p. 204 of vol. xlviii. has failed entirely, and reasoned in a circle: he first forms *arithmetical* progressions, which he could not find in any Series, and then shows, that the inseparable property of arithmetical progressions attends such series, of his own forming. I wish likewise, that some of those fond of contemplating Musical Ratios and Intervals, would consider the *complete Harmonic Series* XVII, XV, XII, X, VIII, VI, V, 4, III and 1, whose ratios are given in p. 385. What would be the effect on the Ear, of a Chord thus completely filled, on Mr. Liston's Euharmonic Organ?."—Editor.

LXXX. *On the Ratio which exists betwixt the Velocities of Bodies in motion, in Fluids; and the Power required to maintain such Velocities.*

To Mr. Tilloch.

Sir, — In The Philosophical Magazine for March last (p. 200,) a number of queries occur, and upon the second of these I take the liberty of offering some short remarks.

Mr. George Rennie states, that "it has been found that the ratio of resistance (of bodies moving in fluids) being as the squares of the velocities does not maintain; or in other words, that a quadruple power will not produce a double effect or velocity."

The first part of this sentence does not, I apprehend, admit of the construction implied in the latter; the *resistance* being no measure of the power, unless the circumstances of time and space are taken into consideration along with it. A quadruple power will doubtless produce not only a double, but a quadruple *effect*; but this effect, so far from producing a *double velocity*, ought only to increase it in the proportion of about 10 to 16, or more correctly as $\sqrt[3]{1}$ to $\sqrt[3]{4}$; being as the *cube roots* of the power: as I shall endeavour to explain.

It is ascertained from the experiments of Burt and others, that the *resistance* is very nearly as the squares of the velocity; or, for example, that if a body A, immersed in water, be put in motion by means of a cord passing over a pulley, and having weights suspended therefrom; these weights will produce velocities in A proportional to their *square* roots. Now let the body A, and consequently the weight, move at the rate of 1 foot per second, and let the weight capable of producing this be = 1 lb. The *power* then is equal to 1 lb. falling through one foot *per second.* But let A now be required to move at the rate of 2 feet per second; the resistance being quadrupled, the weight will require to be increased in like ratio and equal 4 lbs. and this weight will now descend at the rate of 2 feet per second, and the power is therefore equal to 4 lbs. falling through 2 feet per second, or *eight* times what was required in the first case.

It is therefore obvious, that if the *resistance* is as the *square* of the velocity, the *power* required to overcome that resistance must be, as that resistance involved into the velocity, or as the *cube* of the velocity.

I am, sir,

Your most obedient servant,

Glasgow, May 6, 1817. Henry Creighton.

LXXXI. *De-*

LXXXI. *Description of an Instrument by which the Moon's Distance from the Sun or a fixed Star may be cleared from the Effect of Refraction and Parallax;—also of a new Reflecting Goniometer.* By Mr. J. B. Emmett, *of Trinity College, Cambridge.*

My object in describing this instrument is to facilitate, if possible, in some degree the method of finding the longitude by a lunar distance, principally for the use of the masters of our merchant-ships, by abridging the calculations.

I do not offer the instrument as complete and perfect; it requires a greater number of observations than is desirable, and itself is not very simple. As, however, it will most probably receive successive improvements and ultimately assume a more convenient form, and hast he principle is rater new, it may not be totally unworthy the attention of the mariner. The principle is this:

Let HQO (Fig. 1, Plate IV.) represent the plane of the horizon, ZM*m*O a vertical circle pssing through the true and apparent place of the moon; Z*s*SQ another vertical circle passing through those of the star; MS is their true and *ms* their apparent distance: let *hqo* represent a circle in the plane of the horizon, ZPO, *zhq*, circles in the planes of ZM*m*O and Z*s*SQ; join MC, *m*P, SC, S*p*: the diameter *ho* must be supposed evanescent compared with HO, .: P, *p* and C must be supposed coincident. An observer at C will perceive the objects at M and S; but if a prism be placed at P which elevates any object seen through it in the direction ZPO, the angle of deviation equal MP*m*, and another depressing vertically placed at *p*, having an angle of deviation equal S*p*S, the rays which reach the observer's eye will be PC, *p*C, proceeding from the real places M and S, a sextant would then measure their true distance, and its plane would be held in the plane MP*p*S passing through the observer's eye, and their true, and not *m*P*p*S their apparent places. It is now very easy to perceive that if two quadrantal arcs be fixed upon the frame of a sextant capable of moving upon axes which always meet in the centres of these arcs, such arc having a prism of a proper angle of deviation, the refraction taking place in the plane of this arc, the rays from the moon passing through one prism, and those from the star through the other; by placing the axes of these arcs in such a position that the angle between them shall equal the true distance known within a few minutes from the longitude by account and estimated from the Nautical Almanac, then elevating the arcs till from the point of intersection the arcs measured to the plane of the instrument equal the true altitudes of the objects,

jects, a distance taken with this sextant will be cleared by observation.

Fig. 2, represents the addition to a common sextant; A the index mirror; B the horizon glass; NO the telescope; GLM is a circle having B for its centre; QPG is a quadrantal arc attached to the tube FG, whose axis is the axis of motion of this arc. This tube is fixed to another, D, placed before the horizon glass by a brass frame CEF; at D within the tube is fixed a prism, its refraction taking place in the plane of the arc QPG: the prism has such a shape that only the reflected rays can pass through it. At G is another prism for the star, whose refraction is in the same plane, but in the opposite direction. HK is another arc capable of being carried round the circle by an index below, and moving upon a joint K, the direction of the axis being a line joining K and B.

In taking an observation with this instrument, take the altitudes of the objects; at the expiration of about five minutes, take them again; from the change of altitude their real altitudes will be known at the end of the next five minutes, which will allow sufficient time to make every preparation. Knowing the longitude by account, we can ascertain the true distance between the moon and star within a few minutes, which is sufficiently near for our purpose. Move the index bearing HK, till the arc KG equal true distance known nearly. Elevate the two quadrantal arcs, till from the point of intersection R the arc RK, and from the same point RG, shall equal the true altitudes of the moon and star respectively, at the expiration of the second five minutes. Then at the end of this time measure the distance, which will be the real one; for since KG = MS the true distance nearly, RK = ZM = moon's true altitude, and RG = ZS = the star's true altitude; the spherical triangle KRG = ZMS = zPp, .: a line joining R and B if produced would pass through the zenith when the two objects are brought into contact, .: RG and RK will be two vertical circles, and the prisms will elevate and depress vertically.

In order to adapt the instrument to all possible values of the quantity of refraction and parallax, it may be accompanied with a number of prisms having different angles, and which will bring the object nearest to its true place; and finding the difference between the apparent distance and the distance thus corrected by direct :: ascertain the true distance. Thus let SM (fig. 4) be the true distances; *sm* the apparent; let the prism bring the objects into the position S′M′, when Mm and sS are very small; MM′ : M′t : : M′m : mt′. This will, however, only be an approximation to the truth, which will seldom deviate so far as to introduce an error of importance.

A more

A more correct use of the prisms is this: let them be capable of being inclined to the rays which fall upon them; then the angle of deviation may be changed, by which means a number of prisms will not be required. The best method of applying the prisms is to make use of two compound ones; let AB, BC (fig. 5) represent two prisms, the base of one joining the vertex of the other; let DE be the ray incident upon AB, on leaving the prism, its direction would be FGK, but after passing into the other prism, is elevated, taking the direction HL; by varying the angle ABC, or by inclining both the prisms differently to the incident ray, the real angle of deviation LMD may $= o$, and from o increase to any required quantity. This latter method does not require that the prisms be ground to any particular angle, for any two prisms having very small vertical angles, equal or unequal, may be used.

With respect to the circle GLM, if it be divided so far that three or four minutes may be read off, it will be sufficiently accurate, and in the arcs HK and QPG the degree may be divided into six equal parts; each very distinctly visible through a lens of one inch focal distance, and greater accuracy than this is not essential.

Having only described the principle of the instrument, I purpose in a future Number of this Magazine, to describe the mechanism minutely, and investigate the different formulæ required; also to ascertain the error in distance arising from any given error in the computed distance, or altitudes.

Description of the Reflecting Goniometer.

This little instrument was first invented for the purpose of measuring the angles of the prisms adapted to the sextant which I have just described; being in some respects more convenient, and certainly capable of measuring an angle with a greater degree of precision than most goniometers at present in use, it may not perhaps be unacceptable to the mineralogist.

ABC (fig. 3) represents a graduated brass circle; in its centre is placed a plain mirror D at right angles to the plane of the circle, capable of being turned round by a nut below the instrument; HK is an index carrying a vernier; at a right angle to this is fixed a brass pillar at a right angle, to which is attached a small spring tube G, through which passes a rod of brass, having a plate of brass E, fixed at one extremity, and a milled nut at the other F.

The application is obvious; the mirror is adjusted, as in a Hadley's sextant; the crystal is attached to the plate E, by a little

little gum, the vernier is brought to zero, and the nut F turned round till two adjacent faces of the crystal are at right angle to the plane of the circle; the mirror D is then turned round till on viewing any object by reflection, the image reflected by one face of the crystal is coincident with that reflected by the mirror; then the mirror and one face of the crystal are parallel; the index (which of course carries with it the crystal) is next to be moved, till another face of the crytal is brought parallel to the mirror; the angle through which the index has been moved is the supplement of the angle of the crystal.

The advantages of this instrument are these: 1st, since when the plane of the mirror and one face of the crystal are parallel, the two reflected images will be coincident, in what position soever the eye be placed; the goniometer may be held in the hand when in use. 2d, As both the mirror and the crystal may be adjusted with the utmost precision, and the coincidence of the two images accurately observed; the angle may be measured very minutely. 3d, It is very portable.

LXXXII. *New Outlines of Chemical Philosophy.* By Ezekiel Walker, *Esq. of Lynn, Norfolk.*

[Continued from vol. xlviii. p. 345.]

There is scarcely a single operation of Nature that has been explained in such a manner as to meet with general approbation. Electricity, combustion, respiration, animal heat, fixed caloric, radiation of cold*, composition of water, and various other objects of physical inquiry, are supposed by philosophers in general to be clearly explained; but conclusions in direct opposition to those have been advanced by others. Now it is evident that either one or both of these explanations are erroneous, and therefore terrestrial physics cannot be ranked with the sciences.

When an erroneous hypothesis is recorded in books of science and taught by the most learned professors, few will be inclined to doubt of its truth. But the first step to knowledge is to doubt. He who never doubts never examines. And without examination erroneous opinions may be handed down from one generation to another, if not detected by what is called *chance*.

Let us not, therefore, follow those who, from habit, indolence, or the fear of singularity, are the determined supporters of old established opinions; but examine those opinions with due care, and then mark the progress of future discoveries.

In the investigation of problems in terrestrial physics, it is necessary to observe, that there are certain invisible, imponderable, untangible

untangible elements in matter, which are only known to us by the effects which they produce.

There are two modes or ways of investigating physical problems. Some are investigated by experiment only; but in others experiment and observation are the data we have to reason from. As examples of the first kind of investigation, let two experiments be taken from electricity.

It has long been a disputed point among electricians, whether glass be permeable by the electric fluid, and whether electricity by induction be permanent.

To investigate these problems by a single experiment, I took the Florence flask described in a former paper*, and suspended it by a short brass chain from the ceiling of a room. When the electricity of the earth was very strong, I took a glass tube about 3-4ths of an inch in diameter and two feet long; and after having excited it by a silk rubber, I held it under the flask at the distance of about two feet: the gold-leaves within the flask instantly began to show signs of electricity; and on gradually carrying the tube up towards the flask, the divergency of the leaves increased.

When the tube was brought to the distance of about nine inches from the flask, the leaves diverged to an angle of sixty degrees, and continued electrified for fourteen days. The tube was not in this experiment brought nearer the flask than nine inches. This experiment shows by mere inspection, that the electrical element met with very little obstruction in its passage through thin glass; and that electricity by position or induction is permanent.

In making delicate experiments on electricity, it is necessary to attend to the electricity of the earth; for, if that state of the earth had been very weak at the time when the above experiment was made, a very different conclusion might have been drawn.

But in reasoning on experiments and observations, in deducing general conclusions from them, it is of the utmost importance to observe:

1. That all the circumstances or natural things concerned in the experiment be accurately known, and taken into the investigation; and,

2. That no hypothetical opinion be taken for granted.

In the experiment to show the decomposition of water, there are three things concerned in it; that is to say, the two electrical elements,—thermogen the element of heat, photogen the element of light and water. These being the only circumstances

* Phil. Mag. vol. xlvi. p. 210. Exp. 10.

concerned

concerned in the experiment, it remains to show how the two gases are formed.

The three circumstances contained in this experiment admit only of two combinations, as the elements in this instance do not combine—for where they do they cease to act on matter. Hence thermogen and water form oxygen gas, photogen and water form hydrogen gas—water being the only gravitating matter.

But suppose for a moment that water were a compound of oxygen and hydrogen, then the circumstances or natural things would be 4, and their combinations $2 \times 2 = 4$. Thermogen and photogen being united to the supposed component parts of water, each combination would produce a new compound; thus,

Thermogen combined with the oxygen of the water, would form a compound (*a*). Thermogen combined with the hydrogen of the water would form another compound (*b*). Photogen combined with the oxygen of the water would form a third compound (*c*). And photogen combined with the hydrogen of the water would form a fourth compound (*d*).

But in the experiment of the decomposition of water, we have only two compounds, oxygen and hydrogen gases; consequently the supposition is erroneous.

In the decomposition of water there can be no more than three circumstances concerned in the experiment to produce the two gases; therefore, if water be a compound of oxygen and hydrogen, the electric fluid (as it is called) is a simple element; but if the electric fluid be composed of two elements, then it follows that water is a simple body.

Now, as it has been proved by the most satisfactory experiments that the electric fluid is composed of two invisible elements*, water is unquestionably a simple body, and the only ponderable base of the two gases.

This is not a new conclusion; but it is derived from principles very different from those made use of by Dr. Priestley and other eminent philosophers. Dr. Priestley proved by several chemical experiments, that water is a simple body, and supported the opinion that it is the only ponderable base of oxygen and hydrogen gases and other aërial fluids.

The supposed discovery that water is a compound body, received its chief support from the following experiment. M. Berthollet and his associates burned together fifteen grains of inflammable air and eighty-five of vital air, and obtained exactly one hundred grains of water; in which by decomposition they found again the same principles, and in the same proportion.

* Phil. Mag. vol. xliii. p. 281.

Now

Now this very experiment proves that water contains nothing that is combustible; for of all combustible bodies when burned with some substance containing the element of heat (oxygen), the inflammable matter is converted into light, the oxygen into heat; and what remains at the end of the process is an incombustible substance. Had the water been a compound of two gases perfectly combustible, according to the French doctrine, they would have been wholly converted into light and heat, and no water would have remained ; or, had any part of the water been combustible, the weight of the water remaining at the end of the experiment would have been so much less than the weight of the two gases. And the two elements of combustion, being imponderable, add nothing to the weight of the two gases ; and hence the reason that the weight of the water found at the end of the experiment was exactly equal to the weight of the two gases.

Since light, heat, and an incombustible substance are produced by burning together the two gases, it is necessary that combustion should be investigated before the composition of water can be understood from this experiment.

The learned translator of Haüy's Treatise on Natural Philosophy* observes, that "the subject of combustion is a very difficult one, respecting which opinions are still afloat; the theory of the eminent yet unfortunate Lavoisier, though adopted by Haüy, and indeed by the generality of chemists, both continental and English, is far from satisfactory."

And after giving some account of the theories of M. Lavoisier and Dr. Thomson, he further observes: "still it must be acknowledged that this is a very obscure subject; the reader would, therefore, do well, before he forms a decisive opinion, to trace the progressive improvement of the theory in the hands of Boyle, Hooke, Mayow, Beecher, Stahl, Scheele, Kirwan, Black, Crawford, Lavoisier, Brugnatelli, and Thomson, with a view to which he may advantageously consult the article *Combustion* in Dr. Gregory's Encyclopædia, and in the Supplement to the Encyclopædia Britannica; Robison's Life of Dr. Black ; the Edinburgh Review, No. 5 ; Nicholson's Journal, N. S. vol. ii ; Nicholson's Chemistry; and Thomson's Chemistry†." Hence it appears that the French academicians were totally unacquainted with combustion, when they made their experiments from which they deduced the conclusion, that water is a compound body.

As the term phlogiston has occasioned much controversy, it may be necessary to remember what has been stated in a former paper‡ ; that the imponderable element of hydrogen gas of Lavoisier, the phlogiston of Stahl, Scheele, and Priestley; the ne-

* Published in 1807.

† See also Phil. Mag. vol. xlii. p. 363, for a more recent theory.

‡ Phil. Mag. vol. xlvi. p. 430.

 gative

gative electricity of Franklin, and the element of light, or photogen, are only different names for the same thing.

And the fire-air of Scheele, the dephlogisticated air of Priestley, and the oxygen gas of Lavoisier, are only different names for the same gas; the elementary or imponderable part of which is the same as the positive electricity of Franklin, and the element of heat, or thermogen.

That one of these is the element of light, and the other the element of heat, may be demonstrated thus: The gas procured from coals, with which the streets of large towns are now illuminated, contains the element of light. This element produces a most brilliant light with very little heat, as it consumes only a small quantity of the oxygen gas of the air. That the oxygen gas of the atmosphere contains the element of heat, may be clearly understood from the use of the blowpipe. This instrument produces the most intense heat, without increasing the light of the lamp; and the same effect is produced by other blowing machines used to increase the heat of furnaces.

Scheele observes in his Treatise on air and fire*, that "phlogiston is a true *element* and a simple *principle*, which enters into so close and subtle an union with empyreal or fire-air that it even penetrates through the most subtle pores of all bodies. From this union arise both the *matter* of *light*, and likewise the *matter* of *heat* †. In all these compositions phlogiston undergoes not the least change; and from the last composition it can be separated again. Phlogiston can by no means be obtained by itself; for it never separates from one body, though united with it ever so closely, unless there be present another coming in immediate contact with it."

Hence the term *phlogiston*, which has been so much animadverted upon by chemical philosophers, is only another name for that inflammable *element* contained in a certain class of bodies, and this is perhaps as proper a name as any other that has yet been invented.

Lynn, May 12, 1817. EZEKIEL WALKER.

[To be continued.]

LXXXIII. *Description of an œconomical culinary Stove. By* RICH. P. PLAYER, *Esq. Malmsbury.*

To Mr. Tilloch.

SIR, — A CORRESPONDENT in your Number for November last has proposed some inquiries respecting the most œconomical construction

* Page 103.

† The *elements* of light and heat are very different from the *matter* of light and the *matter* of heat. This very important subject still remains for investigation.

construction of an oven for baking, &c. and an apparatus for heating liquids and drying substances by steam. Having had a simple, and by no means expensive, culinary stove for more than a year in constant use in my own kitchen, which was constructed without a knowledge of or reference to any other of a similar kind, is very œconomical with respect to fuel, and unites great convenience and cleanliness with extent of application, I have thought a description of it may be worthy of your notice. It has been adopted by several of my neighbours, who have expressed themselves much pleased with it; and though the description does not completely solve the inquiries suggested, it may be useful to your correspondent, or others who may choose to make trial of one of a similar construction:

It is extremely probable that something similar to the apparatus I am about to describe may have been employed by others; but the skilful direction of heat from a small quantity of fire to our various culinary and domestic purposes is by no means generally understood; and notwithstanding the exertions of Count Rumford and others, the attention of men of science to this interesting and useful subject will most probably be still repaid with valuable improvements. I venture to hope, therefore, that the present communication will not be without its use in contributing to improve our domestic œconomy, and diminishing that wasteful consumption of fuel unavoidable in conducting culinary and domestic processes by an open fire.

The stove which I am now to submit to your notice projects in front of the ordinary fire-place, which has been built up, the necessary communications with the chimney being preserved.

The fire-place of the stove is built of bricks; it is nine inches long in the grate, six inches wide, and seven inches deep. The grate is situated four inches and a half from the door, and the interval is covered with a plate of iron. The front of the stove as far as the side wall of an oven on the right is covered with a plate of sheet-iron 1-8th of an inch thick, in which the doorways are cut, and which is secured by stays walled into the brickwork. Over the top of this fire-place a cast-iron plate is laid twenty inches square and 3-8ths of an inch thick.

From the top of this fire-place a flue for the passage of the heated air passes under the oven, then up the side furthest from the fire till it reaches the top, whence it descends down the back of the oven to the bottom of the side next to the fire, and is continued up that side, and then along the top to the centre, where its escape into the chimney is regulated by a sliding register.

The width of the flue round the oven should be about three inches in the part under the oven, and two inches and a half up the side furthest from the fire; the space at the back of the

oven,

oven, which serves for the descent of the hot air, should also be about two inches and a half wide, and likewise that part by which the hot air ascends, seeking its way to the chimney. The other dimension, or the depth of the flue, depends on the size of the oven, the flue being of the whole depth of the oven excepting only the thickness of the brick-work in front, which should not exceed three inches. that the quantity of surface exposed to the hot air may be as little reduced as possible.

The oven which I have adopted is about fourteen inches wide, twelve high, and sixteen deep from front to back, which will be found convenient dimensions for moderate families.

Moveable stoppers are inserted in the brick-work for the convenience of cleaning the flue—one of these, placed at the back corner parallel to the bottom of the oven and top of the fire-place, serves for sweeping out the flue at the bottom, at the back of the oven, and up the side next the fire-place; another at the front bottom corner, furthest from the fire-place, serves for cleaning the ascending flue furthest from the fire; and a third in front over the door of the oven for cleaning the top part of the flue. A door, closed with sheet-iron, opens into the chim ney, over the fire-place, for sending up a sweep to clean the chimney.

At the distance of about three feet over the stove, there is a horizontal frame of wood about four feet long by twenty inches wide, having wires or strings from end to end, three or four inches apart, for drying linen, &c. This frame, by means of a pair of hinges, may be turned back against the wall when not in use.

By this simple apparatus, the various processes of boiling, steaming, roasting, baking, &c. can be conducted on an extensive scale with the greatest ease and in the most cleanly manner, with a quantity of fuel so small as to be scarcely credited by those accustomed only to open fires, and with what are called small-coals, which can be purchased here at about two-thirds of the price of the mixed or large and small together. The same fire which heats the cast-iron plate sufficiently for boiling, steaming, &c., at the same time heats the oven for roasting or baking.

The kitchen is completely warmed in the coldest weather, and can be readily ventilated in summer. Linen, &c. is dried and aired with great rapidity, and flats are heated for ironing. The oven being constantly hot is found useful for various purposes, and when not otherwise wanted, is kept open, or shut, as more or less heat in the kitchen is required. On its top, plates and dishes are warmed for the table, or liquids may be kept at a moderate heat for any length of time. When the fire is not wanted, it may be kept in for several hours, by nearly closing the registers, with very little consumption of fuel, and may be blown up in a few minutes by opening them.

Boiling,

Boiling, steaming, and frying, are conducted on the cast-iron plate over the fire-place. One of the size above described, twenty inches square, is large enough for most moderate families; for, when a vessel is made to boil by being placed on the hottest part, it may be removed to one side, where it will keep boiling for any length of time; and another which is required to boil may be substituted in its place.

In families where large quantities of hot water are wanted, a boiler may be set on one side of the cast-iron plate and an oven on the other,—but this will not in general be found necessary.

Persons accustomed to place their vessels over the irregular surface of an open fire, or on the hooks and other contrivances made use of to support them, will be surprised at the great steadiness and cleanliness with which the process of boiling is conducted on an iron plate.

Steaming may be conducted immediately over the vessels of boiling water, or in others at a distance; or the steam, confined between the sides of double metallic vessels of various forms, may be applied to the purposes of warming water, drying, &c.

The bottoms of vessels used for boiling on an iron plate should be perfectly flat; and both these, and those used for steaming, should have their sides and tops kept as bright as possible.

The boiling temperature in liquids, except the plate be very hot, is not obtained quite so soon as over an open fire; but this will be found productive of little or no inconvenience, as the vessel has only to be placed on it somewhat earlier.

The common frying-pan may be used over an iron plate; but one with deeper sides would be preferable, to prevent the flying over of any particles of fat, which should be carefully avoided, both on the plate and in the oven.

Bread may be toasted by being cut with a smooth surface, and placed on a small wire frame laid on the iron plate, so close as just to prevent its contact. Small pieces may also be toasted in the fire place, and steaks, &c. may be readily broiled therein, on a gridiron adapted to its form and size.

Cheese may be toasted with much greater facility than before a fire, by being placed on the stove in a common plate, and turned once over.

That roasting can be well conducted in an oven, is by no means generally known. The difference between it and baking is this, that the latter process is performed, as your correspondent suggests, with the door closed; and the former, with it sufficiently open to admit of a gentle current of air passing constantly through the oven: and as in every oven, when the door is open, a current of cold air enters near the bottom, and, passing round up the back, returns out highly heated under its top, it is neces-

 sary

sary to place the meat in this upper current of air. The oven door may be kept quite shut till the meat is warmed through, and begins to roast; and then should be opened about the width of one-eighth of an inch in front, or wider if the processs goes on too rapidly. The meat should be turned and dripped occasionally; and generally requires about one-third more time than when roasted before a fire; from which, however, when the process is conducted with moderate care, it cannot be distinguished by the nicest taste.

As the lower current of air tends to preserve the dripping-pan cool, placing it in another containing water, as described by Count Rumford, will not be found necessary after a little practice in regulating the heat of the oven by the registers.

It is obvious that one of Count Rumford's roasters, or roasting ovens, may be heated by the flue from the fire-place, as well as a common cast-iron oven; but as the former in particular are much more expensive, more complex, and less durable—and as the process of roasting can be conducted in the highest perfection in a common cast-iron oven, this, in families where cheapness and simplicity are kept in view, will obtain the preference.

The oven might be constructed of sheet-iron at a trifling expense, and would never be liable to injury from the fire, at the distance at which it is placed. Even an old cast-iron pot, placed sideways, with a wooden stopper, would make no contemptible oven; and with a cast-iron plate, a fire-place door, and a few bars, a stove might be cheaply constructed in those situations of life where comfort and œconomy are more particularly desirable.

As the temperature of the oven can be varied at pleasure, by urging the fire more or less, by keeping its door shut or open, and in the latter case by placing the article to be subjected to the effects of heat higher or lower, it forms of itself a small drying stove; and when in addition to it the iron plate on its top, and that over the fire-place, are taken into consideration, it is obvious that any degree of temperature up to red heat can be always obtained; and in the current of warm air immediately over the stove, when drying at a lower temperature is required, it can be conducted with great ease, and on a more extensive scale.

It will be observed, that the fire-place is not situated immediately under the centre of the iron-plate, but at the distance of about nine inches from its left-hand edge, which, allowing six inches for the width of the fire-place, leaves about five inches in the clear between it and the side-wall of the oven. The object of thus placing it towards the oven is, that too much of the heat may not be expended under the plate, but that a due share of it may be distributed around the oven.

It may, however, be placed immediately under the centre of the plate, in which case the entrance from the fire-place into the first

first flue (which in the present construction is over the upright side of the fire-place) should be made sloping upwards from near its bottom.

It will be found convenient to have a strip of iron plate, three or four inches wide, riveted to the right-hand edge of the cast-iron plate, and turned up at a right angle against the side of the oven-flue, as the mortar is liable to crack with the strong heat, and to be struck off by the culinary vessels.

Once in three or four days a little straw, or a few shavings or sticks of brush-wood, should be kindled in the fire-place, and suffered to burn freely for a few minutes with the dampers open, to clear the flues of soot, which would otherwise collect and impede the passage of the smoke. With this precaution, the stoppers, which are left for cleaning them, will only require to be withdrawn once in a quarter of a year or longer, to remove the ashes of the soot, which is readily done with a brush having a flexible wire handle.

If at any time, for want of burning out, the flues should get so full of soot as to impede the current of the smoke, which only happens from inattention, it may be easily broken down by introducing the brush, and then burnt, without the trouble of sweeping it out.

The high price of what are called the steam kitchens, their complex structure, and the frequent repairs some of them stand in need of, together with the difficulty of heating boilers, ovens, &c. *œconomically,* from an open fire, even were the flues which surround their sides constructed in the most advantageous manner, are objections to their general adoption; not to mention the smoke, soot, and dust of open fires, and the more extensive application to the purposes of drying and warming of which a stove is capable.

I fear I may have trespassed on the patience of some of your readers by descending too much into detail on a humble subject; but I trust to their candour for excuse, when it is considered that my observations are ultimately for the use of persons in the humbler situations of life; that our culinary processes are, even at this enlightened period, commonly conducted with scarcely more skill in the application of heat, than they were by our aboriginal ancestors; that individuals accustomed always to open fires are likely at first to experience occasional embarrassment in the direction of its unseen energies in a more artificial manner; and that the highest speculations of science, however gratifying to our vanity, are comparatively of little value till they are made subservient to our necessities. I am, sir,

Your obedient servant,

Malmsbury, Dec. 10, 1816. RICH. P. PLAYER.

LXXXIV. *A Supplementary Table of Musical Intervals, for supplying the Omissions in Plate V, of our* xxviiith *Volume: making several Corrections therein, and supplying the new Symbols, by which certain of these Intervals are expressed in the Letter-press of our Work, for the Convenience of Printing.* *By Mr.* JOHN FAREY, *Sen.*

Symbols.	Names of Intervals, &c.	New Notation by Σ, f and m.	Indices of Powers of the Primes 2,	3 and	5.	Ratios or Fractions.	Common Logarithms and Reciprocals.
1	Minor FIRST, or Unison....	....	....	....	....	$\frac{1}{1}$..........	·0000000,0000
γ	Lesser COMMA...........	9Σ + m	−26	12	5	$\frac{66,430,125}{67,108,846}$..	9955851,8238 / 44148,1762
β	Minor DIESIS	31Σ + 3m	−18	4	5	$\frac{253,125}{262,144}$.....	·9847951,1860 / 152048 8140
Δ	Greater DIESIS	43Σ + 4m	1	−8	5	$\frac{6,250}{6,561}$........	·9789099,7959 / 210900,2041
E	Maximum DIESIS	53Σ + 5m	−10	−4	7	$\frac{78,125}{82,944}$........	·9740050,5483 / 259949,4517
✠ or ✳✳, or bb	TONE Minimum, or double *Sharp*, or double *Flat*	83Σ + 2 f + 7m	10	−2	−3	$\frac{1024}{1125}$......	·9591474,3419 / 408525.6581
IV	Major FOURTH............	301Σ + 6 f + 26m	5	−2	−1	$\frac{32}{45}$..........	·8519347.6454 / 1480652.3546
5th	Minor FIFTH	311Σ + 6 f + 27m	−6	2	1	$\frac{45}{64}$.........	.8470325,5979 / 1529674,6021
V	Major FIFTH.	358Σ + 7 f + 31m	1	−1		$\frac{2}{3}$	.8239087,4094 / 1760912,5906
6th	Minor SIXTH	415Σ + 8 f + 36m	−3		1	$\frac{5}{8}$	.7958800,1734 / 2041199,8266
VI	Major SIXTH	451Σ + 9 f + 39m		1	−1	$\frac{3}{5}$	.77815 2,5038 / 2218487,4962
7th (or 2-4ths)	Minor SEVENTH, or double minor Fourth	508Σ + 10 f + 44m	−4	2		$\frac{9}{16}$	.7501225,2678 / 2498774,7322
VII	Major SEVENTH	555Σ + 11 f + 48m	3	−1	−1	$\frac{8}{15}$	.7 69987,2793 / 27 0012,7207
8th	Minor EIGHTH............	565Σ + 11 f + 49m	−8	3	1	$\frac{135}{256}$	.[illegible]20938,0319 / 277906[illegible],968
VIII	Major EIGHTH, or Octave..	612Σ + 12 f + 53m	−1			$\frac{1}{2}$	·6989700,0434 / 3010299,9566

In

Corrections, &c.

In the former Plate,

For .. f	Lesser FRACTION	read ..	f,	Subminimum RESIDUAL.
fc,	SEMI-COMMA Major of Rameau,	read ..	t,	RESIDUAL, of Liston's Scale ; see vol. xxxix. p. 419 Note.
(fc),	SEMI-COMMA, Maxime of Ditto,	read ..	r,	Major RESIDUAL, of Rameau.
R,	Major RESIDUAL	read ..	R,	Major FRACTION.
φ,	PRISMA	read ..	φ,	HYPEROCHE Minor.
đ,	DIASCHISMA	read ..	Ꝺ,	Maximum COMMA.
(D),	DIEZE Minimum	read ..	D,	HYPEROCHE Medius:—and make its ratio $\frac{19683}{20000}$.
π,	HYPEROCHE	read ..	π,	HYPEROCHE Major.
ℰ,	Enharmonic DIESIS, ...	read ..	ε,	Minimum DIESIS,—see vol. xxxix. p. 416 Note.
ſ,	SEMITONE Subminimis	read ..	ʃ,	SEMITONE Subminimis,—see vol. xxxvii. p. 274 Note.
δ,	Chromatic DIESIS	read ..	δ,	Major DIESIS.
ƒ,	SEMITONE Minimum	read ..	ƒ,	SEMITONE Minimis.
𝒮,	SEMITONE Minor	read ..	𝔖	(or I') SEMITONE Minor,—see vol. xxxix. p. 414 Note.
𝕾,	SEMITONE Medius	read ..	𝔖	(or I) SEMITONE Medius, or Minor FIRST,—see ditto.
S,	SEMITONE Major	read ..	S	(or 2) SEMITONE Major, or Minor SECOND.
𝐓,	TONE Major	read ..	T	(or II) TONE Major, or Major SECOND.

For c, read c (or I'): For P, read P (or I'):—For t, read t (or II'):—For 𝕋, read 𝕋 (or II'):—In the 3rd, for 101Σ, read 161Σ:—For FOURTH, read Minor FOURTH. Expunge all the line beginning "COMMA and HALF," as being here unnecessary.

LXXXV. *On*

LXXXV. *On the* Nomenclature *of* Musical Intervals, *and the Advantages of a Set of Symbols or Characters, by which the mutual Relations of the principal ones can be expressed, in the form of simple Equations:—with a Table.*

By Mr. John Farey, *Sen.*

To Mr. Tilloch.

Sir, — In Plate V of volume xxviii. of The Philosophical Magazine for 1807, the first attempt was made, with which I am acquainted, at collecting together in any publication, all the most essential *Musical Intervals;* accurately defining these by means of their Ratios and Logarithms, and assigning to each a *Symbol* or Character, by the help of which their mutual relations might be shown in a great variety of ways, in simple Equations* ; and by means of which Symbols the values of all other Musical Intervals might in like manner be accurately expressed.

Since this period, the publication of The Rev. Henry Liston's "Essay on perfect Intonation," for explaining the principles and use of his Euharmonic Organ, has served to greatly extend and methodize our knowledge of Intervals; and my occasional hours of leisure or relaxation, have since been devoted to an inquiry, into the values of all the Intervals within an Octave, which can arise, in the 3422 combinations, *by pairs*, of which Mr. Liston's 59 Notes and their Octaves, are capable.

The extended and complete Table of Mr. Liston's Intervals thus formed, is found to contain 220 different Notes, two of which are found repeated 52 times†, two others 47 times, two others 45 times, each (all of these being *Concords*) and others in lesser degrees of repetition, down to 12 other Intervals, which are each found only *once*, in comparing every two of Mr. Liston's Notes, and their Octaves, as already mentioned.

In searching for the *principles of Nomenclature*, by which all these, and even a still greater variety of musical Intervals may be each correctly *named*, I have found, that fifteen other Intervals, not mentioned in the former engraved Table in your Work, will now be necessary, each to be furnished with its *Name* and *Symbol*, making in all 48 tabular Intervals: also, that several of the former Symbols, which had been adopted from the late Mr. Overend's Manuscripts, require now to be altered, for adapting them

* Of which use of these Symbols, very numerous examples may be seen, under the names of the various Musical Intervals, in Dr. Brewster's "Edinburgh Encyclopædia."

† Each one of these 59 Listonian Notes, having a true Vth above it and a true 4th below it, except these 7, viz. C′*, C**, C′**, D′, E′♭, F′♭, and E′*. And it is somewhat remarkable, that all of these fall in the lower half of the Octave from C to c.

to

to the common letter-press Printing: and likewise that several of these Intervals, whose Names were selected or given by Mr. Overend, should now be changed, for others, better adapted to classification, and a general Nomenclature.

All these additions and corrections, I have included in a Table to accompany this (see pages 360 and 361). The first column of this Table contains the *Symbols* distinguishing the Intervals, whose *Names* follow, in column 2.

In column 3, the values of the Intervals are given, in the new Notation which I had the pleasure of first presenting to the Public, in p. 140 of your xxviiith volume: the terms of this notation are, the *Schisma* Σ, lesser *Fraction** f, and Most *Minute* m, in the former engraved Table†; and each expression in this column, coupled with the symbol in the first column forms a simple Equation, thus, $\gamma = 9\Sigma + m$, $\mathrm{IV} = 301\Sigma + 6\,f + 26m$, &c.

Column 4, shows the *indices* of the powers of the musical *Primes* 2, 3 and 5, composing the Ratios; the negative signs (—) therein, indicating reciprocals, or the *denominators* of the *Fractions*, which follow in column 5; thus, in the 5th, —6 2 1, indicates the fraction $\frac{3^2 \times 5}{2^6}$, $= \frac{45}{64}$; &c.

The *reciprocal* Logarithms in column 6, are placed lowest, and are the complements of the *Logarithms* to 1; they denote the same Interval, reckoned *downwards*, as the Logarithms do *upwards*; the first 7 figures of the logs. are pointed off, conformably to the common Tables of logarithms.

The *Corrections* in the second page of the Table (page 361), are, I think, sufficiently explained therein, except the compound Symbols I\`, I′, II\`, II′, &c.; wherein, the *grave* accent indicates, the subtraction or fall of a major Comma c $(11\Sigma + m)$; and I\`

* It will be seen in the second page of the Table now sent, that I propose to change the Name, of this smallest -but -one of known Intervals, to *subminimum* Residual; because the class of *Fractions*, as I wish now to constitute it, has always a *negative* sign to the f's therein. Instead of mine being called a Notation by Schismas, lesser Fractions, and most Minutes; it might be said to use, minimum Commas (Σ), subminimum Residuals (f), and subminimum Commas (m), in expressing Musical Intervals, in their utmost varieties.

† In this Notation, each of the 3422 Intervals, formed between pairs of Mr. Liston's Organ-Notes as above mentioned, have been calculated, including *every possible Interval thereon*, less than an Octave; and as in no single instance, any number of Σs is found combined therein, with more than one combination of f and m, it is hereby demonstrated, that for every purpose of expressing the values or comparing Mr. Liston's Intervals, f and m may be omitted, and Σ's only used *with perfect accuracy!*, under the denomination of *artificial* Commas. In p. 420 of your xxxixth volume, I could not venture to speak *thus positively*, after having in the preceding page, given the Σ's or Artificial Commas, answering to each one of Mr. Liston's 59 Nots.

and

and II\`, are to be read thus, viz. the grave major *First*, and grave major *Second* respectively, and so of 3\`, III\`, 4\`, &c. In like manner, the *acute* accent indicates, the addition or rise of c(11); and 1′, I′, 2′, II′, 3′, &c. are to be read thus, viz. acute minor *First* (or acute Unison), acute major *First*, acute minor *Second*, &c.

The sixteen NUMERALS, viz. 1(0), I(47), 2(57), II(104), 3(161), III(197), 4(254), IV(301), 5(311), V(358), 6(415), VI(451), 7(508), VII(555), 8(565), and VIII(612); or minor First, major First, minor Second, &c. (containing 0, 47, 57, &c. Σ's or *artificial Commas*, respectively), are made the *Basis* of the Nomenclature of Intervals within one Octave, which I am now about to describe, in every practicable case: and the deviations, in any cases, from one or other of these Numerals, by Intervals falling between them, in point of magnitude, are expressed, by the name of the interval of deviation, used *as a prefix*.

Thus numeral Intervals, lessened by a major Comma c(11), are either called "Grave" Intervals, as already explained, or "Comma-deficient" Intervals; and intervals increased by the same Comma, are either called "Acute," or "Comma-redundant" Intervals; the words *deficient* and *redundant* being thus only used with, or as prefixes to, the *major* COMMA; and *defective* and *excessive* being used, with any other interval of Deviation from the Numerals; except in such cases where ♯(36), or the SEMITONE *minor*, be the deviation, when the words *diminished* or *superfluous* are used in their prefixes; and these last, are also, but with less precision, called *Flat* (♭), or *Sharp* (✻) Intervals; because, as I have shown in p. 375 of your xxxixth volume, the latter terms and marks, instead of always denoting a fall or a rise of I\`(36) as in the above cases; in a less number of instances, they denote the fall or the rise of I(47), respectively; on which account, the single ♭ or single ✻, are *unfitted for a correct Nomenclature of Intervals;* although the double Flat (♭♭), or double Sharp (✻✻ or ✠), from having *always the same value* (viz. I\`+I), may be so admitted, and marked ✠ (83), as in my supplementary Table, page 360.

In page 8 of your xxxth volume I pointed out, that the number of f's in the new Notation for any Interval, usually indicates, the number of equal half Notes or finger-key Intervals, 12 in the Octave, or *isotonic* SEMITONES (51), to which such Interval nearest approaches; but although there are 12 Isotonic Notes, as well as 12 f's in an Octave, yet the number of *Numerals* is 16 in the Octave, as above shown; and therefore, for inferring the Numeral from the number of f's in the new notation for any given Interval, in order *to name* it, other considerations are necessary, which I will now proceed to explain.

By a reference to my engraved and supplementary Tables it will

will be seen, that the number of f's found in the notation of the several Numerals, are as follows, viz.

1(0), I(f), 2(f), II(2f), 3(3f), III(4f), 4(5f), IV(6f), 5(6f), V(7f), 6(8f), VI(9f), 7(10f), VII(11f), 8(11f), and VIII(12f).

Whence it appears, that f, 6f, and 11f, are each of them belonging to the notation of two different Numerals, viz. I and 2, IV and 5, and VII and 8, respectively; and either, or both of these numerals may be used, in naming the Intervals, to which these numbers of f's respectively belong:—for examples, Mr. Liston's Interval E* Gb, being equal $67\Sigma + f + 6m$, is either $I + 2\mathfrak{C}$, or $2 + \mathfrak{C}$; and may be called, either the Double minor-comma-excessive major *First*, or, the Minor-comma-excessive minor *Second*.—Mr. L's Interval B′f, $=300\Sigma + 6f + 26m$, is either $IV - \Sigma$, or $5 - c$; and may either be named, the Schisma-defective major *Fourth*, or, the Comma-deficient minor *Fifth*;—and in like manner, his Interval Dbc, $=555\Sigma + 11f + 48m$, is either VII, or $8 - \mathfrak{C}$; and its name is either, the Major *Seventh*, or the Minor-comma-defective minor *Eighth*, &c.

In all the other number of f's, only one Numeral should, or indeed can with propriety be used, in naming the Interval, to which such number of f's belong.

It will now be proper to consider the classification of the smaller tabular Intervals, which can, according to the principles of Nomenclature now under explanation, be used as above, *as prefixes to the Numerals*, in naming Euharmonic Intervals: these are ten in number, viz. $\Sigma(1)$, $\gamma(9)$, $\mathfrak{C}(10)$, $c(11)$, $\mathfrak{d}(12)$, $\varepsilon(21)$, $\beta(31)$, $f(32)$, $\triangle(43)$, and $E(53)$:

These ten Intervals being, *without any* f *in their notation*, they belong to a particular class of Intervals, all of which might have received the general name of COMMA, if it had been practicable, to find a sufficient number of prefixes, *for distinguishing* between these various Commas; but this not being the case, the expedient has been adopted, of dividing this class into Families, and calling five of the larger of these Intervals DIESIS, instead of Comma.

Thus, we now have in the Tables, the enharmonic $\varepsilon(21)$, the minor $\beta(31)$, the major $f(32)$, the greater $\triangle(43)$, and the maximum $E(53)$, as prefixes to the family term *Diesis*; and which compounds, or even their doubles, triples, &c. may themselves also be made prefixes, to any of the Numerals, when so required, in the naming of particular Intervals.

For an example, Mr. Liston's Interval D* F′b, $=89\Sigma + f + 8m$, is either $2 + f$, or $I + 2\varepsilon$; and may either be named, the Major-diesis-excessive minor *Second*, or, the Double-enharmonic-diesis-excessive major *First*; and so of others.

As

As prefixes to the term *Comma*, when it is retained as a family Name, we now have in the Tables, the lesser γ (9), the minor $\mathfrak{C}$ (10), the major c (11), and the maximum $\mathfrak{D}$ (12); which compounds, or their doubles, triples, &c. may themselves also be employed, as prefixes to the Numerals.

Thus, for example, Mr. L's Interval C′ * G′, $= 289\Sigma + 6f + 25m$, is either IV $- \mathfrak{D}$, or $5 - 2$ c, and may either bear the name of, the Maximum-comma-defective major *Fourth*, or, the Double-comma-deficient minor *Fifth*; and so of others.

The remaining and least Interval of this family, which has been mentioned, might, in order to carry on the analogy, have been called the minimum Comma Σ (1); and in like manner also, m (0) might have been called the subminimum Comma; but I have been unwilling to load these two small Intervals, so important in the new Notation, with compound Names, instead of their present simple ones, of Schisma and Minute.

All the class of COMMAS, which I have been mentioning, as well as a great many other Intervals belonging thereto, by being *without* any f in their Notation, may, like, or analogically to, the most known Interval among these, the major Comma c (11), (which is the difference of a Tone major T, and a Tone minor t) be derived, from deducting some *one* of the several *Tones* ✠ (83), t (93), T (104), or $\mathbb{T}$ (115), (each having *two* f's), from *another* of these *Tones*.

Or, from deducting some *pair* of these several *Semitones*, $\mathfrak{l}$ (25), δ (26), $\mathfrak{s}$ (36), L (46), $\mathfrak{S}$ (47), S (57), P (50), or $\mathbb{S}$ (68) (each having *one* f) from some *one* of these *Tones* (or *vice versa*), in all their varieties of combinations.

It would too much encroach on my present leisure, and on your pages, to enlarge on this mode of deriving the whole class of *Commas*; but the following examples may somewhat serve to explain it, viz. $10\,t + 22\,S - 21\,T = m$, $2T - t - 2\,S = \Sigma$, $4S + t - 3\,T = \gamma$, $2S - T = \mathfrak{C}$, $T - t = c$, $3\,T - 2\,t - 2\,S = \mathfrak{D}$, $2\,S - t = \varepsilon$, $4S - T - t = \beta$, $T + 2S - 2\,t = f$, $2T + 2S - 3\,t = \Delta$, $T + 4S - 3\,t = E$, &c.

The whole class of TONES (or major *Seconds*) thus defined by means of *their two* f's, includes, in fact, a numerous family of other Intervals besides the four which are so named in my Tables; of these we have one other example in the Table (having no m), viz. $\mathbb{R}$, $= 5\Sigma + 2f$; which is $= T - 9c$, or $II - 9c$, and may be called, either the Nine-comma-deficient major *Tone*, or the Nine-comma-deficient major *Second*.

The entire class of SEMITONES thus defined, as containing *each one* f *in its new Notation*, is by no means confined to the six which are so named in the Tables, with the prefixes maximum, major, medius, minor, minimis, and subminimis; and two intermediate

intermediate ones, P(58), and L(46), which are left with their former simple names, Apotome and Limma, for want of more prefixes than are above enumerated.

But there is in the Tables another family of smaller Semitones, which for convenience are named HYPEROCHE, of which there is the major ϖ (15), the medius D (14), and the minor ϕ (10).

And a family of still smaller such Semitones, named RESIDUAL, having no m, with the several prefixes, greater $\mathbb{R}$ (5 ?) *, major $\mathfrak{r}$ (5), Liston's $\mathfrak{t}$ (4), medius χ (3), minor r (2), and subminimum f (0): and to which the Minimum Σ(1) would have been added, but from my desire, to avoid loading this very important Interval, with compound names, which is already so well known by the simple name of Schisma.

A principle, on which all the numerous family of RESIDUALS (of the class of Semitones, but *in which no* m's *enter)* seems to be deducible, from considering certain of the larger of the Commas and smaller of the Diesises, as *quarter Tones;* and *two* of these to be equivalent, to *one* of certain of the *Semitones* respectively; analogously to the modes in which I have shewn in p. 366, that Commas may be derived from Semitones and Tones; but this is a subject on which I must not at present enlarge, except to give such Equations, for the several RESIDUALS in the Tables, viz. $5L-4\mathfrak{S}-2\varepsilon=\mathrm{f}$, $\mathfrak{S}-L=\Sigma$, $3L-2\mathfrak{S}-2\varepsilon=\mathrm{r}$, $2L-\mathfrak{S}-2\varepsilon=\chi$, $L-2\varepsilon=\mathfrak{t}$, $\mathfrak{S}-2\varepsilon=\mathfrak{r}$, and $5L-3\mathfrak{S}-4\varepsilon=\mathbb{R}$.

It remains now to mention, only one other class of Intervals, in my Tables, and which are called FRACTIONS, *wherein* f *is negative*. Three of these have the prefixes, medius d (1 ?), greater F (1 ?) and major R (6).

I have never found leisure, and probably never may, for deducing any general principle, on which these and the numerous other Fractions in my large MS. Tables of Intervals, may all be derived. My Table of Mr. Liston's 220 Intervals (which I hope at some time to be able to publish) contains no example of this class of Intervals called Fractions, which occasions them to be of far less consequence, in my estimation.

I am, sir, your obedient servant,

Westminster, Nov. 29, 1815. JOHN FAREY, Sen.

* The reason of the expression R (5?) being used, is, that this is one of the few Intervals, which my artificial Commas will not *definitively* express, without the use of f and m, or one of them.

*** Mr. *Farey*, when he very lately sent me this Communication, explained the reason of the long delay since it was written, to have arisen, from sending his MS. some time after it was completed, to a musical Friend, who he conceived was much interested in the subject it treats of, and of whom he requested the favour to revise it, and mark any suggestions for its improvement which might occur to him; but this was never done, or the Paper returned to Mr. F. until lately, without comment, in the state it went, and now appears.—EDITOR.

LXXXVI. *Sum-*

LXXXVI. *Summary of Meteorological Observations made at Paris during the Year* 1816*.

The thermometer which is kept at the Royal Observatory is exposed to the north, at about eight metres (24 feet) above the ground, and in a wooden case around which the air circulates freely. The graduation has been verified by that of an excellent instrument constructed by M. Gay Lussac himself, with all the precaution necessary to secure the utmost exactness. The degrees have in all our tables been reduced to those of the centigrade scale.

It appears to result from the recent researches of M. Humboldt, that in all climates we may, without the risk of any sensible error, take as the mean temperature of every day, the mean of the extreme temperatures. The minimum being observed at sunrise, the maximum about three o'clock P.M., the half of the sum of the indications of the thermometer at these two periods, for all the days of each month, will furnish the numbers which express the mean temperatures of the twelve months of the year. In the table which follows, we have placed the results of 1815 by the side of those of 1816. It will be seen from these, that in regard of temperature, these two years differ much less than we might have been led to suppose. The extremes of the thermometer in 1816 have been : $+28°$ July 20th, and $-1°,8$ Feb. 11th.

Temperature of the Vaults of the Observatory.

The observation of the thermometer, in subterranean situations, has acquired great interest, since geometricians have demonstrated, that at a sufficient depth the temperature, under every latitude, ought, abstracting accidental causes, to be the mean of the temperatures of the surface. A glance at the following table will show, that at 28 metres (85 feet) under the ground (which is precisely the depth of the vaults of the Observatory) the diurnal and even the annual variations of the thermometer are altogether imperceptible.

At Paris, the mean of the temperatures of the surface does not appear to surpass 10° 6′, that of the vaults is 12° 1′ : it is difficult to assign the cause of so great a difference.

Hygrometer.

Our hygrometer has been constructed by Richer : it is placed in the shade, and towards the north. In the subjoined table we have only noted the observations made at three o'clock P. M., which, making allowance for accidental circumstances, is every day the period of the greatest dryness. On referring to the separate

* From the *Annales de Chimie* for Dec. 1816.

separate tables, it will be observed with surprise, that in this year so remarkably rainy, the hygrometer fell nevertheless so low as 34° in April, the term of saturation being 100°. M. Ramond informs us, in his work, that he never saw, even on the Pyrenees, the hygrometer below 39°, except under very rare circumstances.

Quantity of Rain.

The rain is collected at the Observatory in a cylindrical recipient placed on the platform of the building at 30 metres (90 feet) above the ground. It passes from that into a tin vase, which is closed on all sides, and where it cannot experience any sensible variation: and care is besides taken to measure it a short time after it has fallen.

From the average of eight years of observations, (since 1809,) the annual quantity of rain at Paris appears to have been 48·46 centimetres (17 inches 11 lines). The result of this year (1816) is equal to 54·54 centimetres (20 inches 2 lines). That of 1815 was only 45·07 centimetres (16 inches 8 lines). The rain collected in 1811 exceeded by 4·4 centimetres (1 inch 7 lines) what has fallen during the year just elapsed; and nevertheless the number of rainy days in 1811 was only 143, while in 1816 there have been no less than 167.

It would appear from some experiments made in England, that the quantity of rain is more or less considerable in proportion as the gauge in which it is received is elevated above the ground. The new instruments with which the Observatory has been lately enriched will enable us, in the course of the ensuing year, to appreciate the amount, and perhaps to assign the cause of these differences.

Months.	Mean temperature.		Temperature of the Vaults.		Hygr. at 3 P. M.	Rain in Centimetres.	
	1816.	1815.	1816.	1811.	1816.	1816.	1815.
January .	+ 2°,6	— 0°,6	12°,093	12°,087		4c,90	1c,73
February .	+ 2,0	+ 7,3	12,098	12,088	72	0,60	3,14
March . .	+ 5,6	+ 9,6	12,092	12,090	69	4,38	4,06
April	+ 9,9	+ 10,3	12,092	12,093	55	1,28	3,03
May	+ 12,7	+ 14,7	12,093	12,092	65	3,80	2,90
June	+ 14,8	+ 16,0	12,092	12.092	63	5,37	7,87
July	+ 15,6	+ 17,6	12,092	12,092	67	9,67	3,19
August . .	+ 15,5	+ 17,7	12,092	12,092	64	5,07	1,50
Septemb.	+ 14,1	+ 15,5	12,092	12,091	71	6,34	3,18
October .	+ 11,8	+ 12,2	12,091	12,092	74	2,06	6,17
November	+ 4,1	+ 3,5	12,091	12,086	81	4,17	3,67
December	+ 3,7	+ 1,8	12,091	12.090	83	6,90	4,63
Averages	+ 9,37	+ 10,48	12,092	12,090	70	54,54	45,07

Barometer.

The determination of the mean pressure of the atmosphere on every place of the earth is the most important object to be attained by the observation of barometrical variations. In occupying ourselves with this inquiry we have followed the course which M. Ramond has traced in the excellent work which he published in 1811. All the heights have been corrected by the effect of the temperature, and reduced to zero by employing $\frac{1}{5412}$ as the coefficient of the dilatation corresponding to a degree centigrade. The observations made at the same hours have been collected in vertical columns, and the averages reckoned by decades and by months. From these calculations it appears, as M. Ramond has already deduced from his observations at Clermont, that the barometer is subject in our climates as well as at the equator to a periodical daily oscillation, the operation of which, though often masked by accidental variations, becomes manifest when a sufficient number of observations are combined to compensate the fortuitous effects of disturbing causes. It has also been found that the mercury attains its greatest height at nine o'clock A.M. and that it continues afterwards descending until three o'clock P.M.; when it remounts and attains its second maximum at nine or ten o'clock at night, after which it redescends to repeat the following day the same course of fluctuation. If we might be permitted to reckon from the observations of one year the exact compensation of accidental causes, it would seem to result from the averages of the different hours exhibited in the subjoined table, that the diurnal oscillation diminishes in proportion as the latitude increases. Thus under the equator the extent of this sort of atmospheric tide reaches to two millimetres, according to the researches of Humboldt; and three years of observations at Clermont-Ferrand (lat. 45° 47′) make it only about one millimetre; while at Paris it has not altogether exceeded seven-tenths of a millimetre. Ulterior observations will, perhaps, settle this point: those of which we have here given a summary prove already, that the heights corresponding to the different hours of the day differ sufficiently the one from the other, to show that the thing, as M. Ramond has remarked when proposing to determine the mean pressure of the atmosphere in a given place, cannot be arbitrary.

Table

Table of the mean Height of the Barometer in 1816.

Months.	9 A.M.	Mid-day.	3 P.M.	9 P.M.
	m.m.	m.m.	m.m.	m.m.
January .	752,61	752,19	752,06	752,16
February .	756,71	756,71	755,92	756,21
March . . .	753,88	753,79	753,02	753,27
April	749,92	749,64	749,06	749,18
May	753,50	753,40	752,96	753,51
June	755,21	754,94	754,56	754,62
July	751,22	751,05	750,67	750,91
August . .	756,70	756,29	755,68	756,03
Septemb.	756,21	756,11	755,66	756,35
October . .	754,37	754,15	753,39	753,68
November	753,52	753,64	753,53	754,03
December	755,68	755,46	754,95	755.12
Averages.	754,13	753,94	753,45	753,76

Miscellaneous Remarks.

During this year there have been at Paris 167 days of rain, 13 of snow, 19 of hail, 71 of frost, and 10 thunder-storms. The wind has blown from the north 12 days; from the north-east 51; from the east 24; from the south-east 24; from the south 52; from the south-west 83; from the west 84, and from the north-west 36.

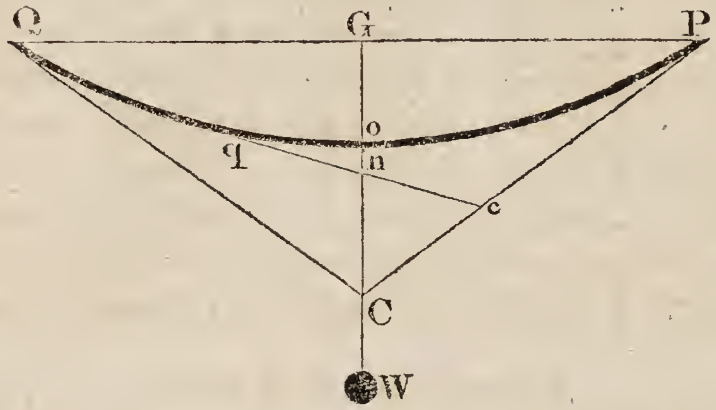
LXXXVII. *On Chain Bridges. By* A Correspondent.

The proposed bridge at Runcorn having greatly excited the attention of the public, and the consideration of mathematical and mechanical men, who seem to differ considerably as to the principles as well as the construction of a chain bridge; for the information of persons immediately interested, and those who have curiosity and ability to prosecute these inquiries, I have collected into one point of view all the essential parts of this difficult subject, by which calculations may be made, and without which no project of this kind, however plausible, ought to be entertained.

It is too evident to need demonstration, that if a chain consisting of a number of connected links be suspended by one of its extremities, the lowest link will suffer the least strain, the next link will be drawn by its own weight, and that immediately

below it; and so of all the rest, the last link being strained by all appended from it, in the same way as if so many equivalent distinct weights were hung from it by separate strings. By parity of reasoning, if the chain be moved from the vertical to a leaning or curving position, similar consequences ensue; but not according to the same law, the stress varying as the links or bars are inclined to each other.

To set this matter on an incontrovertible basis, the following abstract proof of the particular case under consideration, (and which is the easiest that occurs,) viz. when the points of suspension are in the same horizontal line, and the chain of uniform construction, is adduced: If P*o*Q represent a curve formed by



a heavy flexible chain or other thing suspended horizontally by its ends at P and Q, then it is demonstrated* that the total effort exerted by the chain on the fixed points P and Q is the same as if a weight W, equal to the chain, or the chain and its *uniform* load, were suspended from the point of concourse C of the two tangental springs PC and QC: and that if a tangent *qnc* be drawn to any other point of the curve, as *q*, then it will be, As the sine of the angle *qn*C is to the sine of the angle QCW, so is the pressure at P or Q to the tension at *q*; or, as G*nq* and QCG are the supplements of the angles *qn*C and QCW, it will be, As sine of *qn*G is to sine of QCG, so is the pressure at P to the tension at *q*: but the angle *qn*G is greater than the angle QCG (Euclid xxxi. 1.); consequently the second term of the ratio being less than the first, the fourth term will be less than the third, (Simson's Euclid, b. v. prob. A.) or the tension at *q* less than the pressure at P. I have been more particular on this point than may seem necessary, because an author who has some pretensions to mechanical knowledge has asserted that the tension at the lowest point *o* is greater than at P; contrary to this demonstration and article 551 of MacLaurin's Fluxions, where it is also proved that the tension at *o* is to this tension at *q* as the fluxion of the ordinate to the fluxion of the curve; consequently that at *q* is

* Barratt's Mechanics, p. 100; Gregory's Mechanics, p. 133; Bridge's Mechanics, 326; Emerson's Miscellanies, 163; Playfair's Outlines of Philosophy, 233.

greater

greater than that at o, and greatest of all at the points of suspension.—Hence we learn, that to make the strength of the chain in proportion to the stress, which reason and science require, its dimensions must vary from the lowest point o to P. This increase in size and weight will change the curve from a catenarian to one of a more complex and transcendental nature, which none but the most transcendent abilities will be able to develop. We therefore must content ourselves with the approximate knowledge derived from considering the chain of invariable dimensions, which is a problem of difficulty and labour, as is well known to mathematicians; and none other need attempt it. I shall not give the elementary investigation by which the following equations and a knowledge of the subject can be derived; it may be seen in the authors before quoted, some of whom have given examples of numerical calculation, which may be useful to those who have not laboured much in the difficult parts of science, but may be desirous of ascertaining the results of the present or some future project of this nature, putting a to denote the stress or strain at the lowest point o; $Go=x$; $GQ=y$; and $Qo=z$. The following are the general equations alluded to:

1st, $y=a\times\text{hyp. log.}\ \dfrac{a+x+\sqrt{2ax+x^2}}{a}=a\times\text{hyp. log.}\ \dfrac{z+\sqrt{a^2+z^2}}{a}$.

2d, $z=\sqrt{2ax+x^2}$.

3d, $x=-a+\sqrt{a^2+z^2}$: from which, if any two of the quantities be given, the rest may be found. But it is evident, from the transcendental nature of the curve, that this can only be done in the present form, by the tedious method of trial and error, a mode of operation not suited to common calculators. To obviate this difficulty in some degree, Dr. Hutton, at p. 33 of his Principles of Bridges, has reduced the first equation into the following series:

$a=\frac{1}{2}x\times\dfrac{y^2}{x^2}+\frac{1}{3}-\dfrac{8x^2}{45y^2}+\dfrac{691x^4}{3780y^4}-\dfrac{23851x^6}{453600y^6}$, &c. "where a few terms," he says, "are sufficient to determine the value of a pretty nearly." Perhaps the above series will be better adapted to calculations, by substituting $v=\dfrac{y}{x}$ and reducing the fractional coefficients of the series into decimals. Thus $a=\frac{1}{2}yv^{-1}\times: v^2+\frac{1}{3}-\cdot1777v^{-2}+\cdot1852v^{-4}-\cdot05137v^{-6}$, &c. a being found, the length of the chain and other particulars may be obtained by substituting the value of a and x in the second general equation. If the length of the chain and distance of the points of suspension be given, the value of a may be found, by trial and error, or in a series in terms of y and z by a similar process to that pursued by Dr. Hutton.

 Having

Having now the value of a, the stress at the lowest point o of the curve; the distance of the points of suspension; the length, weight, and swag of the chain, &c.; the tangent QC, and subtangent GC, become known from the equations

$$QC = \frac{y}{a}(a + x);\ GC = \frac{yz}{a};$$

(see Emerson's Fluxions, page 205), whence (by Bridge's Mechanics, page 302), as 2CG is to CQ, so is the whole weight of the chain, &c. to the pressure of the point P or Q, or, as $\frac{2yz}{a}$ is to $\frac{y}{a}(a + x)$, so is $2z$ to $a+x$. Hence it appears that the stress at a the lowest point is to that at suspension, as a to $a+x$!

As a denotes the tension exerted at the lowest point by a weight equivalent to a feet in length of the chain and the weight appended; the pressure at P will be equal to a weight of the bridge of $a+$GO in length.

Thus we ultimately obtain the stress at o, and pressure at the points of suspension, and the strength of the chain necessary to support the whole bridge and its appendages, and its ability to resist every strain that may occur from loaded carriages, or other fortuitous causes. These, likewise, are subjects not of conjecture, but of equally rigorous and difficult calculation. As the particular dimensions and circumstances are not yet given, it is unnecessary to introduce them on supposition.

Cor. 1. The greater v is, the fewer terms will be necessary to find the value of a; in practice $\frac{1}{2}yv^{-1}(v^2+\frac{1}{3})$ (or perhaps $\frac{1}{2}vy$) will give it sufficiently near.

Cor. 2. If x be very great in comparison with y, the value of a will be very small, as it evidently is when the chain is nearly in a perpendicular position: and the pressure at P is nearly equal the whole weight of the chain.

Cor. 3. If x be small in respect to y, the strain at a is immense, and when x is $=o$, infinite; showing that it is impossible by any definite force to draw a chain or cord perfectly straight. This is fully confirmed by common experience.

Cor. 4. If x and y be equal, the stress at o is $=\frac{2}{3}y$, and at P $\frac{5}{3}y$ nearly.

Cor. 5. When y is a mean proportional betwixt the length and swag of the chain, the stress at the lowest point is nearly equal to the weight of the chain.

The superficial and practical mechanic may, perhaps, smile at the preceding tedious and difficult mode of proceeding; but I assure him, there is no easier way known of obtaining the requisite information: experience, his guide, can give him no assistance in

in this case of extraordinary difficulty and perfect novelty; nor can experiment on a small scale furnish any analogy, as similarity in the catenaries is impossible:—theory, therefore, combined with a knowledge of the strength of materials, is the only source of satisfactory conclusions. At some future period, when the dimensions, &c. are specifically determined, I may exemplify the preceding by the numerical calculations, by experiments, and additional observations, to excite attention, and stimulate superior abilities to give a complete comprehensive view of this important proposition in mechanical philosophy, with the easiest rules of computation, and the most approved methods of construction: some curious and useful deductions may be drawn from the preceding; but I omit them, beseeching the reader in the words of the poet,

> " ——— si quid novisti rectius istis,
> Candidus imperti; si non, his utere mecum."—Hor.

N. B. The preceding was written and printed previous to the meeting of the Committee at Liverpool, when a Plan and Report were given by Mr. Telford, of the most eligible mode of accomplishing this desirable object. And nearly a similar one from Captain Brown, which may receive some further notice. J. F.

LXXXVIII. *Notices respecting New Books.*

Dr. Wilson Phillip, of Worcester, has in the press, and will speedily publish, An Experimental Inquiry into the Laws of the Vital Functions, with some Observations on the Nature and Treatment of Internal Diseases; in part republished, by permission of the President of the Royal Society, from the Philosophical Transactions, with the Report of the National Institute of France on the Experiments of M. Le Gallois, and Observations on that Report.

LXXXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 1. Sir E. Home communicated a paper containing some remarks on the nature of generation and utero-gestation. After stating that the process of Nature in this case had hitherto eluded the observation of the most enlightened physiologists, he observed that accident alone had enabled him to develop some new, and, as he conceived, decisive facts on the subject. A female servant having left her master's house a few hours on the 7th of

 January,

January, returned, and was observed to be more than usually gay in the evening, having received some present. A few days after she was seized with a fit, and died on the 15th. Her body was opened, and an impregnated ovum was found in the uterus; but it was so extremely small, that although the uterus was macerated in spirit of wine, it would have escaped observation, had not Mr. Bauer with his powerful microscope discovered and delineated it. His drawing and description accompanied Sir Everard's remarks. The two parts destined to become the head and heart were distinctly visible; and Sir E. considers this a demonstrative proof that, contrary to the received opinion, impregnation may take place without the existence of the catamenia. He says that every time animals are in heat an ovum passes from the ovarium into the uterus; and he related a case of a woman having had three children (being married at seventeen) previous to menstruation. In the case alluded to, however, it appears that the woman was at sea, a situation not altogether natural.

May 8. Sir E. Home furnished some additional remarks on the nature and effects of an infusion of *colchicum autumnale* and *eau médicinale* on the human constitution in cases of gout, and their effects on animals. He observed that the sediment of the *eau médicinale* is excessively drastic and severe on the constitution, while that of the infusion of *colchicum* is about half the strength of the former; and that the clear tincture of both is equally efficacious in curing gout, without being so dreadfully destructive to the animal constitution. The result, therefore, of these new experiments is, that the clear fluid, either of the vinous infusion of *colchicum* or of the *eau médicinale*, may be taken with equal advantage to the health, and much less injury to the body; but that of the *colchicum* is much milder.

T. A. Knight, esq. F.R.S. detailed a series of experiments on the shrinking of timber; he made various sections of different kinds of wood, in order to ascertain in what direction it should shrink most, and found that the greatest contractions always take place next the medulla. He drove cylindrical pieces into the medullary parts of various trees, and, on exposing them to heat, invariably found that, however tightly driven at first, they always fell out of the wood. To this circumstance he ascribes the cracks or clefts observed in oak and other trees, which have been hitherto ascribed to the effects of frost.

May 15 and 22. A letter from Dr. John Davy to Sir Humphry Davy was read, containing an account of many new and curious experiments and observations on the temperature and specific gravity of the sea, made during a voyage to Ceylon. Dr. Davy is disinclined to believe that the zones have any peculiar temperature.

temperature. It appears that the temperature of the sea is generally highest about noon, and is higher during a storm; but in this case the period of highest temperature is somewhat later. One observation seems highly important, namely, that shallow water is colder than deep ; and that, in consequence of this difference of temperature, seamen may discover at night when they approach either shoals, banks, or the shore. Dr. D. found that on approaching the coast the water was always two degrees colder than when in the open sea. He enumerated the causes which influence the temperature of the ocean, such as tempests, currents, and the solar rays. Many other remarks and experiments mentioned in this letter may prove useful to navigators. The author, however, strongly recommended the general use of the thermometer to seamen, by means of which he thinks many new and highly important phænomena may still be discovered.

Mr. Sewell, of the Veterinary College, in a letter to the President, stated his having discovered a method of curing horses which are lame in the fore feet. He observes that many fine horses, in consequence either of being over-driven, or their own ardent spirit, often become lame in one of the fore feet, and are consequently consigned to the cart or the butcher. It occurred to him that this lameness might originate in the nerves of the foot, near the hoof; and in consequence he immediately amputated about an inch of the diseased nerve, taking the usual precaution of guarding the arteries and passing ligatures, &c. By this means the animal was instantly relieved from pain, and the lameness perfectly cured. Mr. Sewell has already cured three horses by this operation. The Society then adjourned till the 5th of June.

BATH LITERARY AND PHILOSOPHICAL SOCIETY.

April 28. Dr. Wilkinson stated to the Society, that the properties of arsenic, of which so little is as yet known, had occupied much of the attention of the late Professor Tennant of Cambridge; and that previous to his death he had informed Mr. Gregor, an eminent chemist in Cornwall, that from an analysis he had made of wootz or Indian steel, he had ascertained that its peculiar hardness arose from a combination of arsenic.

An account of some experiments by Colonel Beaufoy, on the strength of different kinds of wood, was read, and a diagram of the apparatus employed, exhibited. In these experiments the different pieces of wood were secured to a firm block of timber at one end, and the weights were applied at the other end, to which a quadrant was also attached, by means of which the angles of curvature could be ascertained till a fracture took place.

The

The first experiment was made on a square piece of Dantzic oak cut into twenty-five pieces, each piece five feet long and two inches square. The mean weight to produce fracture was 167½ lbs.; the variation being from 114 lbs. to 214 lbs. The section which included the heart of the tree bore 154 lbs., and the strength of the other sections diminished in proportion to their distance from the heart. The angles of curvature did not correspond to the weights employed; in one piece which bore 196 lbs. just before fracture, the angle of curvature was 6° 12′; another which supported nearly the same weight, viz. 193 lbs., only bent to an angle of 5° 25′; while a third piece which broke with considerably less weight formed an angle of 70° 12′.—These variations in elasticity appeared to be determined by the central relationship of the respective sections of wood; the heart section being the least, and the outer section the most elastic.

It appeared further from Colonel Beaufoy's experiments, that a piece of wood divided into any number of pieces loses strength nearly in the proportion of the square root of the number of pieces: thus the original piece of Dantzic oak here employed, would have supported five times more weight than the sum of the weights of the twenty-five pieces into which it had been divided.

Experiments were made on timber spliced three different ways, and on an average the splicing diminishes the strength nearly one-half.

On a comparison of four different kinds of wood: viz. pitch-pine, English oak, Riga fir, and Dantzic oak, Colonel Beaufoy found the pitch-pine the strongest; and taking the mean of its power of support at 1000, the relative strength of the other woods may be stated as follows:

Pitch-pine	1000	Riga fir ..	782
English oak	923	Dantzic oak	663

The weight of Riga fir being to that of English oak as 659 to 1000, whilst it strength is as 846 to 1000; it is therefore preferable in dry buildings to oak for beams, both on account of weight and expense.

Additional experiments, it was stated, had been made on this subject by Mr. G. Smart, carpenter, Westminster Bridge, from which it appeared that there was a remarkable difference in point of support between timbers lying loose at their ends, and timbers carefully coaked down on plates of buildings instead of being dovetailed, as is often done in carpentry. A lath lying loose at both ends, bent like a hoop and broke with eleven pounds weight, while a similar lath, which was rendered tight at each end by shoulders, supported no less than 240 lbs. Mr. Sepping, in

in his recent improvements on ship-building, has profited much by attending to this important difference.

Some cylindrical pieces of iron ore from Marlborough Down, commonly known by the name of thunderbolts, were exhibited to the Society. They have a stalactitical appearance, and are radiated in their structure. By some they are supposed to be pyritical. On analysis they are found to contain manganese as well as iron, and when exposed to fire become of a reddish cast, arising from the additional oxidation of the iron. The pieces exhibited were a species of the brown hæmatitic iron ore.

Mr. Rotch made some ingenious observations on lights and shades, and illustrated the same by several curious experiments made with four pieces of paper, so arranged and cut out, that by the different intensities of light passing through, such a variation of light and shade was produced, as to form the most beautiful and picturesque landscapes. A gentleman had lately shown Mr. R. a paper so cut, that the shadow formed on the wall presented an excellent representation of the *Salvator Mundi* of Guido.

The meeting concluded by Dr. Wilkinson's examination of the different theories which had been advanced for explaining the phænomena of the Leyden phial. The Doctor endeavoured to show from a variety of experiments, that the results could not be reconciled with the theory of Dr. Franklin, or that which supposes electricity to be a compound of two species, positive and negative, or vitreous and resinous. The communication of Dr. W.'s own theory was promised to be made at the next meeting.

ROYAL ACADEMY OF SCIENCES AND LITERATURE OF BRUSSELS.

The Academy at a recent meeting proposed the following question:

"*What are the applications which can be made of steam in domestic œconomy, and in manufactures, as a means of heating?*"

On this question several memoirs have been presented; on examining which the Academy have awarded the prize of the gold medal to one by M. de Hemptinne, physician at Brussels, as being the most distinguished both for practical detail and theoretical illustration.

The following questions are proposed for competition during the year 1818:

1st. What are the particular defects of the sort of bricks made in this country (Belgium)? What means can be adopted to render them more perfect? What are the materials and processes employed in the northern provinces of the kingdom, for the fabrication

brication of certain species of bricks, the manufacture of which has failed with us?

2d. Is it possible, from satisfactory experiments, or reasons deduced from the doctrine of determinate proportions, to establish with certainty that the radical of muriatic acid is a compound body; or is it more probable that the radical is a simple body? In case of no decision on the question, Which of the two methods (experiment or doctrine of determinate proportion?) is best calculated to simplify the theory of chemical facts?

3d. The printing paper of France, and drawing paper fabricated in England, having an acknowledged superiority above those of other countries, it is required to know in what that superiority consists?—whether the causes are of a local nature, or depend on the materials or mode of manufacture?

4th. What is the quantity of water discharged by a river at a given place, and during a fixed period of time, the breadth, the depth, and declivity of the river being known?—and what are the variations which, at the same point, and during the same space of time, will take place in that discharge, by diminishing progressively, by artificial constructions, the breadth of the river?

The prize for each of these questions is to be a gold medal of the weight of twenty-five ducats. The memoirs to be written in Latin, French, Dutch, or German, and given in—those on the first three questions, before the 1st February 1818, and that on the last, before the 1st Nov. 1818.

XC. *Intelligence and Miscellaneous Articles.*

VENTILATION OF COAL-MINES.

A PAPER on the ventilation of coal-mines, with a plan of the present mode of ventilation, and a plan of a proposed improvement, have been received. It may appear presumptuous in any one not a colliery-viewer, to form even an opinion on the modes best adapted for ventilation; but, notwithstanding, we have formed an opinion, and we are convinced that the method usually followed at Newcastle is not the best that might be adopted. The paper that has reached us might improve the system a little; but till we see some demonstration of the inefficacy of that proposed by Mr. Ryan, we think it would be wrong in us to lend our aid to recommend others less effectual. It is asserted, that any one of the mines at Newcastle might be properly ventilated on his plan, at an expense under 100*l.* A mere denial of the fact, and much less silence, on a subject involving the safety of so many lives, will not convince those who have examined this subject, that the managers of mines at Newcastle can be justified in any longer refusing to be instructed.

STEAM

STEAM ENGINES IN CORNWALL.

In our abstract from Messrs. Lean's Report for March, published in our last, there were some errors. The second and third paragraphs should have been as follows:

Woolf's engine at Wheal Vor, loaded 15·4 per square inch in engine cylinder, lifted 35,759,081 pounds of water one foot high with each bushel of coals.

His engine at WhealAbraham, loaded 15·1, lifted 46,681,105 pounds; and his other engine at the same mine, loaded 3·49, lifted 19,390,210 pounds with each bushel.

According to Messrs. Lean's Report for April, the following were the respective quantities of water lifted one foot high with one bushel of coals, by the engines annexed, during the month.

	Pounds of water.	Load per square inch in cylinder.
24 common engines averaged	22,067,102	various.
Woolf's at Wheal Vor	43,468,554	15·4 lib.
Ditto, Wheal Abraham	44,147,208	15·1
Ditto, ditto	24,674,490	3·81.
Ditto, Wheal Unity	34·057·685	13·1.
Dalcouth engine	42,900,035	11·2.
United Mines	39,244,354	16·6.
Wheal Chance	41,804,141	13·

The intelligence that a committee of the House of Commons had been appointed to take the subject of steam-engines into their consideration excited great alarm in Cornwall, which may be called the land of steam-engines; as it was apprehended that prejudiced or interested people might press their opinions upon the committee, and, taking advantage of the alarm excited by the late accident at Norwich, induce some legislative interference, without allowing time for proper investigation. The fear, considering the property that is at stake in Cornwall, was not unreasonable, but we are well assured will prove groundless. A committee of the House of Commons is not likely to be easily imposed upon by partial statements. The one alluded to has had a laborious task to perform; but, whatever might be the prejudices with which their minds might be filled in common with many others who had been told that steam-engines are *infernal machines,* they have bestowed so much patient investigation on the subject, that the possessors of steam-engines need be under little or no alarm about parliamentary interference.

Any interference of this kind would be injudicious, and prove highly injurious to the progress of national improvement. In this view of the question, nothing could be more natural than that the miners in Cornwall should have felt an uncommon degree

gree of interest in this question. For the property embarked in these undertakings is immense, and the slightest interference with any of their machinery might have thrown many thousands of labourers entirely out of employment, and have caused such a complete inundation of the works as would have required many years to recover any of them, and have destroyed some of the deepest mines entirely. In consequence, the miners of Cornwall deputed some of their number to wait upon the committee, and with them Mr. Lean, the gentleman who is employed by the miners as a body to survey all their steam-engines, and register the work performed by each. In fact, such was the general alarm, that, could the state of the mines have admitted of their absence, the managers of the greater part of the Cornish mines, there called Captains, would have come to town in a body. One of them, Captain William Davy, reckoned one of the most intelligent miners in Cornwall, in answer to a letter from a gentleman in London, informing him of the attempt made by some interested or prejudiced individuals to *put down* all engines working with steam of high temperature, sent him the following:

"Redruth, May 17, 1817.

"Dear Sir,—The contents of your letter have excited no small degree of alarm in this quarter,—but we have still such an idea of the intelligence and information of a committee of the House of Commons, as to encourage a hope that the legislature will not be advised to adopt any measure which might operate to destroy most valuable and extensive property, or even to shut the door against further improvement, by any regulations whatever; for all restrictions tend to fetter invention and limit the powers of intellect.

"With regard to the benefits which have resulted to the mines from the œconomy of fuel introduced by working with steam of high temperature, a pretty correct opinion may be formed from the monthly Report made by Captain Thomas Lean, who is appointed by the adventurers [proprietors] of the different mines for that purpose, and paid by them for performing that duty. These Reports may be relied on. He keeps the keys of the different counters*, and no other person can have access to them. Our friend Mr. —— can show you a set of these Reports.

"Had there been no recent improvement for drawing water from the deep mines,—that is, were we left still to the employment of only the common condensing engine,—I know not a single deep mine in the county that could have been now in existence as a working concern: and to stop any one of them at this moment would be productive of the most distressing consequences;

* The counters are a piece of clock-work, which, being worked by the steam-engine, registers the number of strokes which it performs.—Editor.

for

for at this moment we are all driven to our wits end to find employment for the poor; and were our mines thrown idle, it is impossible to anticipate the result.

"As to danger from engines employing steam of high pressure, none who are competent to give an opinion on the subject,—and none else have a right to do so,—will so far commit themselves as to say that there can be any in working steam of from 35 to 45 pounds pressure on the square inch in the boiler—which is the ratio at which Woolf's engines are worked—provided safety valves be employed, and the construction and materials be of a proper quality. But in fact, this applies also to the common engine—nay, to every machine of high power employed by man.

"As to the common engine being less dangerous than those employing steam of higher temperature, the idea is quite groundless; for those who make them proportion the strength and substance of their materials accordingly,—nor do they ever give their boiler the same strength for a pressure of six or seven pounds which they would for a pressure of forty. In point of fact, I have known several instances of accidents in consequence of the boilers of common engines giving way,—I mean such accidents as have caused the loss of lives,—but we have had none of this kind from Woolf's boilers. By the bursting of a common wrought-iron boiler at Poldory mine, three men were killed and three badly scalded: at Chasewater, two horses were killed by the explosion of the boiler, though at work in a different building from the boiler house. There was also a serious accident at Crenver from one of the common boilers, which caused the loss of several lives. I could name several other accidents of this kind; and therefore I cannot but reprobate the idea of any interference which would *compel* us to employ these engines or boilers in preference to others.—Dear sir,

"Yours sincerely,

"WM. DAVY.

"P. S. Some gentlemen will set off to-morrow for London, to attend to this business."

As several steam engines, besides those to which Mr. Woolf's name is attached in Messrs. Lean's monthly Report, appear to be doing considerably more duty than the common engines have been supposed capable of performing, a correspondent requests that Messrs. Lean will, at the bottom of their monthly Report, or through the medium of The Philosophical Magazine, state the cause or causes of this improvement. Has the increased work been produced by employing steam of high temperature, suffered to expand in the working cylinder, and then condensed, on the leading

leading principle of Woolf's engine? or from improved boilers?—and if so, in what does the improvement consist?—or from these and other causes combined? As particular an answer as possible would at the present moment be peculiarly interesting, as it is known that some questions connected with the use of this most powerful agent now occupy the attention of a committee of the House of Commons.

COLLIER OR MINER LIFE PRESERVER.

We have just seen a very curious contrivance by Mr. Geo. Prior, watch-maker of Leeds, (son of Mr. John Prior, the celebrated Yorkshire mechanic,) which may be considered as a Collier or Miner Life Preserver, and which combines the two advantages of simplicity and efficacy. The object of this invention is, to prevent those accidents which are so frequently occurring from the breaking of the ropes by which corves or buckets are let into the coal-pits or mines. To effect this purpose, iron pins are introduced into the upright frame on which the corfe slides. The apparatus to which the corfe is fixed is furnished with a powerful spring catch on each side, which, without causing any friction in the ordinary working of the corfe, opens out the moment the rope breaks, and, fixing itself on the iron pins, causes the corfe to be suspended, and prevents the person in it from being precipitated to the bottom of the pit. This apparatus, which is adapted to the machinery now in use by the miners, adds very little weight to the corfe, and may be fitted up at a trifling expense.—*Leeds paper.*

TRIGONOMETRICAL SURVEY.

M. Biot, of the French Institute, well known by his curious researches relative to the polarization of light, has come to this country for the purpose of accompanying Colonel Mudge, the conductor of the Trigonometrical Survey, on a philosophical expedition to the Orkneys. M. Biot is now making experiments on the length of the seconds pendulum, at Edinburgh; while Colonel Mudge, and his able assistant Captain Colby, are measuring a base of verification near Aberdeen. The several operations at Edinburgh and Aberdeen are expected to be terminated about the middle of June; when, it is understood, the party will be joined by Dr. Gregory, of the Royal Military Academy, Woolwich, and the whole proceed to the Orkneys, for the purpose of carrying on simultaneously the requisite astronomical observations connected with the Trigonometrical Survey, and the experiments that relate to the vibrations of pendulums. These joint processes, conducted at so high a latitude, may be expected to throw considerable light upon that curious class of problems which regard the figure of the earth.

AFRICAN

AFRICAN EXPEDITION.

Accounts have been received from Lieut. Campbell, on whom devolved the command of the expedition for exploring the Joliba or Niger river, on the death of Captain Peddie, stating his arrival at the head of the river Nunez, whence he intended proceeding across the mountains towards Bammakoo, the place at which Mr. Park embarked; on the surface of which Lieutenant Campbell and his companions are in all probability at this time.

ERUPTION OF MOUNT VESUVIUS.

Private letters from Naples mention an extraordinary eruption of Mount Ætna, and announce, in a positive manner, that the little town of Nicosi has been covered with lava, and that fears were entertained even for the town of Catano.

EARTHQUAKES.

The following is an enumeration of earthquakes felt in different parts of the world since the 1st of January last:

Jan.	13.	In the Gulf Stream.
	17.	At Chamouni in Switzerland.
	19.	At the same place.
	20.	Same place, and also at Alcocér in Spain.
Feb.	11.	At Chamouni.
	13.	At the same place.
	14.	Ditto.
	18.	At Madrid, Barcelona, Lerida and Saragossa.
March	11.	At Lyons.
	15.	At Chamouni, and Messina in Sicily.
	18.	At Madrid, Pampeluna, and several other parts of Spain.
	22.	At Pampeluna.
	25. 26.	At Frascati, Gensano, and other adjacent places in Italy. One shock particularly violent.
	28.	At Chamouni.
	30.	Ditto.
	31.	Ditto.
April	1.	Ditto.
	2.	Ditto, very violent, direction from north to south.

(day not mentioned.) At Palermo.

NAVIGATION.

M. Locatelli, mathematician of Milan, has just invented a new piece of mechanism (says a Paris paper) by means of which vessels may ascend rivers without the assistance of a steam-engine. The first experiment was made on a small boat, and completely succeeded.

succeeded. The inventor asserts that his plan is applicable even to a man of war, and that it will secure her from the danger of shipwreck. The strength of a single man, or at most that of a horse, is sufficient to put the machine in motion.

PERPETUAL MOTION.

Another to the many supposed solutions of the problem of perpetual motion has just been added by a M. Louis of Valence, formerly captain in the Neapolitan service. He has found, he says, " means to raise a column of water strong enough to force another to the same height, which produces in its turn the same effect. Thus, when the impulse is once given, this machine will perpetually retain its action, if there exists a fluid which does not lose by evaporation, or a material indestructible by use. One may, however, employ a quantity of water sufficient to keep the machine in play for several years. This same machine may be employed, as the impelling power, for the production of various kinds of regular motions. The inventor proposes to adapt a clepsydra to it, and he is convinced that, by means of a basin or reservoir, a private house might derive various advantages from it."

SUPPOSED IDENTITY OF PUMICE WITH LAVA, ETC.

To Mr. Tilloch.

May 27, 1817.

Sir,—On perusing an interesting memoir (in vol. xlvii. of the Philosophical Magazine) respecting the composition of basalt, trap, lava, &c. in which it is asserted that lava, basalt, and obsidian are aggregates of pyroxéne and felspar; it occurred to me that pumice may be the latter substance after having undergone a high degree of calcination.—The facts which support this conjecture are the following:

First, with respect to its constituent parts, pumice has a very considerable analogy with felspar. Klaproth obtained from the pumice of Lipari,

Silica	77·50
Alumina	17·50
Potassa and soda	3·00
Oxide of iron / —— of manganese	1·75
	98·75

Vauquelin found in the *Adularia*,

Silica	64
Alumina	20
Potassa	14
Lime	2
	100

The

The same chemist extracted from the *Amazon stone*,

Silica	62·83
Alumina	17·00
Potassa	16·00
Lime	0·30
Oxide of iron	0·10
	96·23

The *Feldspath compacte tenace* of Haüy, or Saussurite, contains, according to Saussure,

Silica	44·0
Alumina	30·0
Oxide of iron	12·5
Lime	4·0
Soda	6·0
Manganese, potassa, and loss	3·5
	100·0

And Klaproth found that the *Klingenstein porphyr* or sonorous porphyry (var. *F. compacte*) was principally composed of silicas, alumina, and soda.

The quantity of alkali in pumice is much less than in any of the above-named varieties of felspar; but it appears to me that it is by no means unphilosophical to suppose that a part of the alkali in felspar may be volatilized by a long continued heat.

Secondly, both obsidian and pumice "give in fire exactly the same products," and "both these fossils not only accompany each other at Lipari, but likewise frequently occur actually blended."—*Klaproth's Analyses*, vol. iii.

There is a circumstance relative to the origin of pumice, which I own I cannot account for. It is this, that obsidian and lava should contain pyroxéne, while the analysis of pumice-stone does not even indicate the presence of either lime or magnesia, both of which were found in considerable quantity by Vauquelin in his analyses of the pyroxéne of Ætna, and in the granular variety of the same mineral, called coccolite.—Vide *Lucas Tableau des Espèces Minérales*, part i. Paris 1808.

The fact, that obsidian and pumice, though so different in composition, give the same products by heat, may appear to militate against the above ideas; but it seems probable that the felspar only is fused in the experiment, and that the pyroxéne is merely enveloped in the paste formed by the former mineral, as, if an attempt is made to fuse pounded granite, the felspar is vitrified, while the quartz remains in its pristine state enveloped in it.

I shall feel particularly obliged to any of your correspondents who will inform me whether any experiments on felspar have given results favourable to the above hypothesis.

I am, sir, yours respecfully,

Lithophilus.

P.S. In the xlviiith volume of the Philosophical Magazine you have affirmed that the Waterloo or Strand Bridge is *entirely* constructed of granite. This is not exactly the case, as, though granite is by far the predominating material of that structure, it contains a considerable quantity of red calcareous gritstone, and of white, laminated, micaceous sandstone.

*** Our correspondent is right respecting the materials of the Strand Bridge: but we meant only that the principal material was granite; every part of the visible exterior of the structure is composed of that species of stone.—Editor.

WATERS OF BADEN.

M. Gimbernat has found on analysis, that the gas of these celebrated waters is not, as has been generally believed, carbonic acid, but azote. By collecting and condensing the vapours which rise from the principal spring, he has obtained a mineral water which differs essentially in its chemical properties from the water which issues from the rock. It is more gaseous, and more impregnated with animal principles; whence M. G. believes it will be found even more salutary than the baths themselves, into which the water does not flow till after a great portion of its volatile properties has evaporated, in consequence of its high temperature, and its exposure to the air. In this opinion he has been confirmed by the singular fact, that the whole interior of the chimney above the spring, and through which its vapours have passed from time immemorial, has become covered with a thick and greasy tegument, which is evidently of a vegeto-animal nature, and proves the sanatory quality of gas volatilized by thermal heat.

DISTILLATION OF SEA WATER.

M. Freycenet, to whom the King of France has given the command of a scientific expedition, which is preparing to proceed round the world, being convinced from experience of the fitness of distilled sea-water for all potable purposes, has furnished his vessel with a still capable of supplying water to the whole crew, consisting of 120 men. On a trial of this still, which was made in presence of a committee appointed by the minister of marine, it was found that by one killogram (two pounds) of coal they could obtain 6·78 killograms ($13\frac{1}{2}$ pounds) of fresh water of good quality. The water had at first an empyreumatic taste, but

but after being some days exposed to the air, became entirely free from it. The expense of water thus distilled is about the same as that of water embarked; but were it even greater, the many obvious advantages attending a constant command of fresh water at sea, are such as must supersede any consideration of this kind.

PNEUMATIC PROVISION-SAFE.

It is a well established principle, that three united agents concur in the destruction of alimentary substances—air, heat, and water; and that by neutralizing one of these agents the action of the other two is paralysed. M. Foucque jun. of Paris is said to have succeeded in effecting this, by producing a vacuum in an apparatus simple, easily used, and not expensive. He has made his apparatus of two sizes. One, which is intended to be kept in the kitchen to receive the dishes to be preserved, is made of a square piece of hard stone thirteen inches in diameter. In this stone a circular groove is cut, and furnished with mastic (or lute); a cast-metal bell is fitted into the groove, and a hole is pierced in the top of the bell of one line in diameter. The other safe consists of a large earthen pot of a thin consistence, around the mouth of which a luted groove is cut, and a cast-metal bell with a hole in the top fitted into it, in the same manner as in the other safe.

When the substances which it is desired to preserve have been placed in either of these safes, a little spunge is dipped into spirit of wine of 33 degrees, then placed in a saucer upon the eatables, and afterwards set fire to by means of a match. A considerable dilatation immediately takes place, which expels the atmospheric air; and in order to prevent its return into the apparatus, the hole in the top of the bell is quickly stopped with common wax. A small quantity of atmospheric air may perhaps get again into the bell; but not more, it is probable, than the combustion of the spirit of wine, not yet finished, will suffice to decompose, and convert into carbonic acid gas, the preservative property of which is well known.—*Paris Journal.*

ARTIFICIAL CONGELATION.

The interesting experiment, by Professor Leslie, which we announced in our last number, has been successfully repeated by Mr. Stodart. The stone from which he made his absorbent powder was taken from Salisbury Craigs, near Edinburgh: this was pounded and dried; and with it, under an exhausted receiver, a small body of water was soon frozen. On procuring a very low receiver, and preparing a larger surface of earth, the process was accelerated, a larger body of water being soon converted into a cake of ice. Experiments were made with various other absorb-

 ents,

ents, of which pipe-clay was the best, equalizing in intensity the whin-trap itself. The latter, however, when in a state of complete decomposition, will probably prove to be the best material for the refrigerating process. This elegant discovery of the Professor promises to prove equally interesting to the philosopher, and important in its application to the common purposes of life, in every climate. Whether required as a luxury in health, or as a necessary in sickness, ice may at all times be readily procured.

A gentleman at Blackheath has found that alcohol and snow or ice mixed together form an absorbent of such capacity, that the temperature of snow when the alcohol is not very strong is reduced from 32° to 17°.

INDELIBLE RED COLOUR.

A solution of dragon's blood applied with a pencil to white marble penetrates deeply, and the outline traced remains perfect, as the colour does not spread. This solution is found to harden marble to such a degree, that if a piece partially stained be exposed to the action of a powerful acid, so that the surface is eaten away to a considerable depth, the tinted part will stand out beyond the rest. The Greeks, in consequence of its possessing these durable qualities, were in the practice of employing it to point the mouldings of their cornices. The *cymatium* of the temple of Nemesis, at Rhamnus, is all around ornamented in this manner. Where the colour has been applied the parts are prominent; the corrosion of the surface having been by this means prevented. The outline appears to have been first traced with a sharp instrument while the marble was soft.—*Unedited Antiquities of Attica by the Society of Dilettanti.*

ARTIFICIAL WOOD.

M. Menke, of Berlin, has discovered a process for transforming the saw-dust of mahogany into a soft paste, which, on exposure to the air, becomes hard and solid as stone, and is therefore susceptible of taking and retaining the forms given to works in marble, wood, and bronze. It may be made to assume the colour of that metal, or gilt. The articles made of it, such as candelabra, lustres, lamps, vases, statues, ornaments of all kinds for furniture, &c. vie in elegance with the finest works in bronze, and cost only one-eighth of the present price.

ELECTRICAL PHÆNOMENA.

Cover a piece of tinfoil with any hard resinous coating about 1-64th of an inch thick; place it between the rims of the universal discharger, and send the charge of a large jar or a battery through

through it, and the tinfoil will be perforated. Its appearance will strongly indicate a passage from positive to negative.

Send the charge through a piece of polished steel covered in the same manner, and an indentation will be made in its surface surrounded by a prismatic ring, which will present a beautiful appearance through the microscope: on the negative side the indentation will be considerably smaller, and without the prismatic ring. *Γ. Δ.*

It has recently been ascertained, that fogs contain a great proportion of water, but not in a condensed state, being kept suspended by the opposing powers of the electric fluid with which it is charged. A convincing proof of it was afforded by a curious meteorological occurrence in Westphalia, where, the fog being driven by a gentle north-east wind against the trees, the electric fluid was attracted, condensation and congelation took place, and the largest trees were torn up by the roots, by the preponderating weight of ice upon their branches.

LITHOGRAPHY.

In our number for March we gave a brief notice of the origin and practice of the new invention of lithography. We have been since informed, that the first application of it to purposes of usefulness unconnected with the fine arts, was made by the Duke of Wellington in the peninsular war, for the purpose of accompanying the general orders, instructions, &c. with sketches of positions. A recent Hamburgh gazette states, that it has also been introduced into the department of foreign affairs in Russia, and been the means of superseding a great number of copying clerks.

TALAVERA WHEAT.

A few years ago, an English gentleman serving with the army in Spain observed near Talavera a fine variety of wheat, some of the seed of which he brought with him to England. It has very long straw, very long ears, and a fine, clear, thin-skinned grain; it is very prolific; it succeeds when sown either in autumn or in spring; and it ripens three weeks earlier than the common wheat of England, in some of the southern counties of which it has been carefully propagated now for five or six years, and has been found to preserve its original character and peculiar advantages. One farmer at Petworth in Sussex had fifty acres of it this last year; and he is now selling it for seed at ten guineas the quarter. It is preferred by millers to all other wheat, and is much sown at this time of the year.

CHEAP BREAD.

Take pumpkins and boil them in water until it is quite thick, and with this water moisten your flour. Dough mixed in this manner makes excellent bread: its proportion increases considerably, at least one-fourth, and it keeps good a length of time.—E.

Birkenmayer, a brewer of Constance, has made an experiment, which has succeeded, in manufacturing bread from the farinaceous residue of the beer. Ten pounds of this species of paste, one pound of yeast, five pounds of ordinary meal, and a handful of salt, produce twelve pounds of black bread, but savoury and nourishing.

MR. BYWATER ON THE POLYPES.

To Mr. Tilloch.

Sir,—Wishing to encroach as little as possible on your pages, the paper I sent you, and which appeared in your last, was abridged from one of greater length. Unfortunately some ambiguity has thereby been introduced, by retaining a passage in page 284, alluding to some observations which had been expunged from another part of the paper. Your readers are requested to omit the paragraph between line 36 and line 45, which will considerably diminish the obscurity.

At the close of the paper an idea is held out that animalcules may exist in vegetables, and extend their race with the vegetable process. That such is the fact I am strongly persuaded; and it is equally probable, from a variety of phænomena, that the organic structure on which the vegetable process depends, as well as the exquisite design and beauty manifested throughout the vegetable kingdom, are derived from the instinctive influence of this numerous class of active agents. But as an investigation of such an extensive nature is not comprehended in the object of the present letter, I shall wave the inquiry until a future opportunity.

An insertion of these remarks in your next Number will confer another obligation on

Your very obedient servant,

Liverpool, May 12, 1817. John Bywater.

SUN'S PARALLAX.

To Mr. Tilloch.

Sir,—The following memoranda were drawn up by Mr. Burckhardt, Superintendant of the Observatory of the Ecole Militaire at Paris, by desire of Count Laplace: as the publication of them may be of considerable service to science, I send you a copy for insertion in your Magazine.

I am, sir, your obedient humble servant,

Al Raschid.

Note on Observations which should be attended to for the Purpose of determining the Sun's Parallax.

1. There will be an opposition of Mars in December 1817.

This planet will be for some time on the parallel of the 118th star of Taurus, whose mean position on the 1st January 1818 will be Æ 79 31′ 9″·4. Dec. 24° 59′ 32″·3. From the 19th November the difference of declination between the planet and the star will be only eight minutes; and that of their right ascension in time only a few minutes. It will be no difficult matter, therefore, to measure their difference of declination several times each night, which, compared with observations of the same taken in Europe, will give the parallax of Mars.

N. B. It must be remarked, that the star is double; but as the distance between the two stars is small, it will be best to refer the *interval* which separates them to the planet.

2. The same by the inferior conjunction of Venus on the 26th December 1818.

On the 24th November Venus will be in conjunction with σ Capricorni, and their difference of latitude will be only 5 minutes.

On the 19th December the planet will be on the parallel of τ Sagittarii.

And on the 21st December on that of σ Sagittarii.

But as it is not likely that these stars can be easily seen in the daytime, within so small a distance from the sun, it will be better to compare the planet with Antares Æ 16^h 18^m, and Dec. 26° S. on the parallel of which star it will be 28th November.

If these phænomena could be observed in India, and at the Cape of Good Hope, the parallax of Venus might be obtained with great precision. The absolute error of the instrument would be immaterial, provided the planet were always referred to the star.

NEW COMET.

Midwolda, April 13.—Yesterday evening, about fifteen minutes past ten o'clock, a comet was seen in the west, but on account of the dark clouds was not visible above three minutes. Its light so much outshone the stars, that it was not easy to determine precisely in what constellation it appeared. It was in or near Gemini.

The above notice in the foreign journals having led to a very diligent scrutiny of the heavens by the astronomers of this country, the following are the reports of the only successful observations which we believe have as yet been made in England.

To the Editor,—Sir, the appearance of a telescopic comet having been announced in some of the continental journals, I carefully watched for it, with a telescope of the magnifying power

power of about seventy times, on every fair opportunity. On the night of the 2d May instant, during a break in the clouds which enabled me to see the constellation *Leo*, I noticed, just above the star *Regulus*, or *alpha Leonis*, a small luminous speck, the light of which did not resemble that of a common star. On observing it for several nights afterwards, it diminished in size, and is now lost in my telescope. Perhaps some of your readers, who may have a telescope of greater magnifying powers, may discover it even now. I am, sir, &c.

Speldhurst, May 19, 1817. S. R.

P. S. The phænomenon was seen first about 9 P. M. and was situated about 10 E.S.E. of Regulus. The latitude of this place is scarcely 2 E.

Tunbridge Wells, May 18.

A luminous appearance was, a few nights ago, discovered in the constellation of the Lion, which had the appearance of a very distant *comet*. It can only be seen by a telescope of considerable magnifying power. Its place in the heavens is about 10′ NNE of the star *Regulus* in the *Cœur du Lion*.

METEOROLOGY.

A gentleman who has been examining the different meteorological instruments in various parts of England, with a view to ascertain the cause of the apparent errors in the Meteorological Journals, has found one cause to be, that the wind is often erroneously put down. The cardinal points on the weather-cocks are in general put according to the compass, instead of the sun; consequently the north point is about 20° west of due north, and all the others are proportionally wrong. A gentleman at Walthamstow has recently constructed a weather vane, which rings a bell at every turn of the vane by the most gentle change of wind. It is also constructed with a spherical cannister of oil at the top, which can drop continually into the hinge, and will not be exhausted for many years.

M. Van Mons of Brussels, to whom the Philosophical Magazine has been indebted for so many valuable communications, has been nominated by the King of the Netherlands to the professorship of physic and chemistry in the university of Louvain.

In consideration of the great benefits the Duke of York has received from the professional talents of John Stevenson, Esq. of Great Russell-street, Bloomsbury-square, His Royal Highness hass been graciously pleased to appoint him his Surgeon-oculist and Aurist.

Mr.

Mr. A. T. Thomson will commence his Lectures on General and Medical Botany, on Tuesday the 3d of June, at two o'clock, in the Anatomical Theatre, Blenheim-street.

LIST OF PATENTS FOR NEW INVENTIONS.

To Antonio Joaquim Friere Marrere, of Broad-street Buildings in the city of London, merchant, for an invention communicated to him by Luis Coctano Altina de Campos, residing abroad, of a method of making or manufacturing an improved machine or instrument for calculating and ascertaining the longitude at sea. —Allowed six months to enroll the specification.—Dated 29th April, 1817.

To William Collins, esq. of Maize-Hill, Greenwich, Kent, for his improvement or improvements in the composition and preparation of a metal for the manufacturing thereof into sheets or plates, and the application, when so prepared and manufactured, to the preservation of ships, by sheathing or covering the bottoms therewith,, and an improvement or improvements of the chain-pumps used on board ships.—6th May.—2 months.

To Henry Wilms, of Union-street, Lambeth, Surrey, cabinet-maker, for his artificial leg, arm, and hand, on an improved construction.—8th May.—2 months.

To James Gerard Colbert, of Winsley-street, in the parish of St. Marylebone, Middlesex, mechanical watch-maker (in consequence of a communication from a certain foreigner residing abroad), for certain improvements in the method or methods of making screws of iron, brass, steel, or other metals, for the use of all kinds of wood-work.—13th May.—2 months.

To Richard Williams the elder, of Dursley, Gloucestershire, card-maker, for certain improvements in manufacturing of cards for dressing woollen cloths.—13th May.—2 months.

To John Walker, residing at No. 12 Great Charles-street, Blackfriars Road, Surrey, millwright, for his improved method of separating or extracting the melasses or treacle from and out of Muscavado, brown, or new sugar.—13th May.—2 months.

To Archibald Thomson, of Church-street, parish of Christ-church, Surrey, machinist and engineer, for his machine for cutting corks.—17th May.—6 months.

To Robert Salmon, of Woburn, Bedfordshire, gentleman, for his apparatus for the more useful, safe, pleasant, and œconomic use of candles, and also improvements in the apparatus now in use for part of the same ends.—17th May.—6 months.

To William Bound, of Ray-street, Clerkenwell, Middlesex, ironfounder, and William Stone, of Berkeley-street in the same parish and county, brass-founders, for their method of applying certain

certain apparatus for converting the fuel used for heating retorts of a gas-light apparatus into coke or charcoal. —17th May.—6 months.

To Benjamin Cook, of Birmingham, Warwickshire, gilt toy maker, for his improved method of making and constructing rollers and cylinders, both solid and hollow, which will be found useful in various manufactories in this kingdom.—17th May.—6 months.

To William Owen, of Wrexham in the county of Denbigh, cabinet-maker, for his portable table or box mangle, upon a new or improved principle, for the getting up and smoothing of linen, cotton, and other articles and things.—17th May.—2 months.

Astronomical Phænomena, June 1817.

D. H. M.	D. H. M.
1. 2.36 ☽ φ ♐	13. 0. 0 ♂ 80 ♓ * 14′ N
1. 6.21. ☽ σ ♐	19. 5.45 ☽ η ♌
4. 0.46 ☽ ε ♑	20. 0. 0 ♂ 96 ♓ * 9′ S
6. 0. 0 ♀ 113 Mayer * 3′ S	21. 8.30 ☉ enters ♋
6. 0. 0 ♂ 23 Mayer * 13′ S	23. 0 0 ♂ 55 Mayer * 14′ N
10. 0. 0 ☽ in apogee	25. 0. 0 ♂ 58 Mayer * 11′ N
11. 3.50 ☽ ♀	26. 0. 0 ☽ in perigee
12. 0. 0 ♂ 77 ♓ * 11′ S	27. 0. 0 ♂ 110 ♓ * 7′ N

Barometrical and Thermometrical Observations made at Tunbridge Wells, from May 1 *to* 10, 1817.

May	Barometer at 9 A.M.	About Noon.	Evening.	Thermometer lowest during the Night.	Weather.
1	29·48	29·82	....	34°	Cloudy.
2	29·62	29·62	29·63	32°	Clouds.
3	29·59	29·52	29·48	39°	Fair.
4	29·50	....	29·68	32°	Fair.
5	29·74	29·74	29·75	34°	Clear.
6	29·80	29·84	29·90	44°	Fair.
7	29·90	29·86	29·70	41°	Clear.
8	29·54	29·54	29·50	44°	Clouded.
9	29·50	29·48	29·47	40°	Cloudy.
10	29·30	29·17	29·15	39°	Clouded.

Observations.

The prevailing winds have been N. and NE. and the temperature low for the time of year. The sudden elevation of the Thermometer to 70° at 2 P.M. on the 8th was followed by as sudden cold.

The cuckoo and wryneck were both heard as early as May

the

the first, and the swallow and martin both common. The foliage of the trees is nearly as late as last year. The oak and beech have as yet hardly any appearance of frondescence. The chesnut has been out a week. The spring flowers are generally late in bloom about this neighbourhood.

Tunbridge Wells, May 11, 1817. T. F.

Meteorological Observations kept at Walthamstow, Essex, from April 15 to May 15, 1817.

Usually between the Hours of Seven and Nine A.M. and the Thermometer again between One and Two P.M.]

Date.	Therm.	Barom.	Wind.
April			
15	52	30·00	NW.—Cloudy and windy; floating *cumuli* and clear sky; very fine day; star-light.
16	46	29·72	NW.—Sun and wind; stormy wind; clear; clouds and wind; star-light. New moon.
17	34	30·10	N.—Clear and *cumuli;* cold gray windy day; stormy wind; some sun; very dark night; some stars at 11 P.M.
18	38	30·34	N.—Windy and sun, and *cirrostratus*; fine day; star-light.
19	37	30·30	N.—Sunshine; white frost; very fine day; star-light.
20	48	30·40	N.—Sun and *cirrus;* fine day; moon- and star-light.
21	43	30·40	N.—Clear and clouds, and windy; very fine day; moon-light.
22	..	....	E.SE.—Clear and clouds; fine day; moon-light.
23	48	30·40	NE.—Gray and windy; great shower at 2 P.M. and wind; fine evening; moon- and star-light; slight *aurora borealis* E.SE. at 11 P.M.
24	33	30·40	N.—White frost; *cirrostratus* and sun; drops of rain; fine day; cloudy and windy. First quarter.
25	43	30·22	N.—Gray and windy; slight rain; gray day; cloudy.
26	38	30·22	N.—Gray morning and gray day; cloudy.
27	43	29·88	NE.—Sun, and *cumuli*, and great wind; very fine day; *cumuli*, and moon and stars.
28	44	30·11	NE.—Gray and wind; gray; cloudy.
29	47	30·00	NW.—Hazy; cloudy and windy; *cumuli* and wind.

April

April			
30	42	29·76	NE.—Windy and cloudy; sunshine; showers; wind and *cirrostratus* and *cumuli.*
May			
1	43 48	29·76	NE.—Windy and gray, and *cirrostratus*; fine cold day; cloudy. Full moon.
2	44 57	29·99	N.—Clear and *cirrostratus,* and windy; fine day; star-light.
3	43 59	29·98	N.—Hazy, and sun and *cirrocumuli;* and windy; very fine day; dark night.
4	52 57	29·37	N.—Sun, *cumuli* and wind; fine day; bright star-light.
5	42 62	30·00	NW..—Sun and hazy; fine hot windy day; star-light.
6	52 63	30·10	NW.NE.—Sunshine; fine day; star-light.
7	43 57	30·20	NE.—Hot and sunny; and some *stratus* NW. and *cumulostratus* and *cirrocumulostratus;* and *stratus* and *cumuli* and *cirrostratus*, and rain; storm at 4 P.M.; distant thunder; star-light.
8	46 72	29·87	W.—Cloudy, and windy; *cumuli* and wind; dark night. Moon last quarter.
9	49 53	29·87	W.S.—*Cumuli* and wind all day; dark night.
10	51 57	29·60	S.—*Cirrostratus;* gleams of sun; slight showers after 4 P.M.; cloudy.
11	45 58	29·54	NW.—Sun and wind all day; cloudy.
12	47 58	29·33	NW.W.—Wind and clouds; and clear; fine day; star-light; *aurora borealis* at 11 P.M. not steady but variable.
13	43 59	29·75	SE.SW.—Sun and wind; showers and sun; hazy; star-light.
14	46 57	29·77	SE.S.—Showers and sun all day; hazy evening; *red* and *black* sunset; star-light night.
15	47 59	27·76	SE.—Sun and clouds; fine warm day; clear and *cumuli;* hazy; star-light, and some *cirrus.*

The Thermometer hangs out of a western window, from which the early height is taken; and the second time is from a Thermometer hanging out of an eastern window.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1817.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Apr. 15	29	52·	30·04	Very fine
16	new	46·	30·	Cloudy—blows hard from NW.—Hail and rain P.M.
17	1	43·	30·33	Ditto
18	2	50·	30·57	Very fine
19	3	54·	30·54	Ditto
20	4	56·	30·56	Ditto
21	5	54·	30·50	Ditto
22	6	54·	30·47	Ditto—fog.—Wind E in the even^{g}.
23	7	47·	30·39	Fair
24	8	51·	30·39	Ditto
25	9	43·	30·30	Cloudy—rain early A.M.
26	10	47·	30·15	Ditto
27	11	46·	30·20	Ditto
28	12	51·5	30·21	Ditto
29	13	52·	29·96	Ditto—Rain at night
30	14	43·	29·85	Rainy
May 1	full	48·	30·06	Fair
2	16	56·	30·10	Ditto—Very fine
3	17	55·5	29·95	Ditto
4	18	54·	29·06	Very fine
5	19	62·	30·13	Ditto
6	20	57·	30·27	Ditto
7	21	60·	30·36	Ditto
8	22	56·	29.99	Showery—Rain all night
9	23	50·	29·94	Fair
10	24	56·	29·56	Cloudy
11	25	54·5	29·57	Fair—Hail and rain P.M.
12	26	53·	29·40	Showery
13	27	50·5	29·77	Ditto—with thunder

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For May 1817.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
April 27	42	50	43	29·92	37	Cloudy
28	42	55	47	30·01	46	Cloudy
29	48	55	49	29·90	50	Cloudy
30	44	50	41	·75	52	Showery
May 1	40	49	43	·85	41	Cloudy
2	45	55	45	·90	62	Fair
3	46	57	52	·80	53	Fair
4	55	64	49	·95	60	Fair
5	52	60	49	30·14	47	Fair
6	48	57	47	·16	56	Fair
7	50	57	46	·10	55	Fair
8	53	67	52	29.80	57	Fair
9	47	52	46	·70	34	Thunder showers
10	49	57	50	·48	39	Fair
11	50	57	48	·30	37	Fair
12	47	60	45	·32	62	Fair
13	47	55	44	·67	42	Showery
14	48	56	45	·62	46	Showery
15	47	60	46	·76	62	Fair
16	50	67	49	·85	82	Fair
17	51	64	51	·80	45	Fair
18	53	69	55	50·	36	Rain
19	50	52	40	·41	21	Rain
20	44	50	45	·45	0	Rain
21	47	47	45	·35	0	Rain
22	46	55	44	·40	31	Fair
23	47	58	45	·43	42	Fair
24	48	60	47	·45	49	Showery
25	50	59	46	·17	22	Showery
26	47	60	50	·23	39	Cloudy

N.B. The Barometer's height is taken at one o'clock.

XCI. *On the Strata of Northumberland and Durham.* By *Mr.* Westgarth Forster.

To Mr. Tilloch.

Sir,—I lately by accident (not being a constant reader) met with your very excellent Magazine, vol. xlv. art. 17, in which I have the gratification to find some encomiums made upon my Treatise on a Section of the Strata, commencing near Newcastle-upon-Tyne, &c. by Robert Bakewell, esq., for which I beg leave to return my grateful acknowledgements.

I have also taken notice of some queries, by a correspondent, with the signature of "A Constant Reader," (art. 19, vol. xlv.) respecting the occurrence of the grindstone-sill. In reply; he will find the stratum, which is quarried for grindstones on Gateshead-fell, at No. 10 on my section, and denominated by the colliery sinkers, brown sandstone or post; the other, he will find by the provincial name of grindstone-sill, not from its being quarried for grindstones, it being a coarser stratum. It probably derived its name from its resemblance to the above; but it is a distinct stratum, and far below the Newcastle one; and is nearly the uppermost bed, on Cross-fell, Kilhope-head, Ramgill-head, Shorngate-cross, &c.

I likewise observe, that the same correspondent is desirous to know, where the iron-stone balls are lodged. Now, by reference to my section, he will find *a plate-bed* where they frequently occur, immediately above the *tumbler beds of the great limestone*, marked 326 on the sectional scale, and commonly called by the miners *cat-heads*; which may very probably have "bivalve shells" inclosed, although I do not perfectly recollect observing them.

Nat. John Winch, esq. states, that my section has been furnished from a similar engraving published at Carlisle (and signed *Wm. Millot miner, which must be Miller*) in the year 1800, and likewise from a section of the coal-strata sunk through at Byker St. Anthony; together with that of Sheriff-hill colliery, printed in Hutchinson's History of Durham. I must vindicate myself by stating to Mr. Winch, that indeed I did see Miller's section previous to my publication; but so far from making any use of it, I did not copy a single stratum from it, as I found on examination that it was very incorrect; and, I believe, has been taken in a great measure merely by computation. However, I do not wish to claim the priority of either Miller's, or any other section of the coal-measures: but this I must state, that my section was entirely made from my own observations, and admeasurements, at several mining fields, and bassets of the strata,

 excepting

excepting the Newcastle coal-strata, which was from information and sections sent me from my friend Mr. Thomas Fenwick of Dipton.

I beg leave further to add, that I hope Mr. Winch will allow that mine is the first attempt to form a connexion between the Newcastle coal and the metalliferous limestone formations; which I trust on examination will be found to be an approximation to truth.

By inserting the above in your valuable Magazine, you will much oblige,

Sir, your most obedient,

Garrigill Alston Moor, May 27, 1817. WESTGARTH FORSTER.

XCII. *Experiments relative to the Action of Hydrochloric Acid upon Mixtures of Tin and Antimony. By M.* CHAUDET*.

THE very lively action which hydrochloric acid exerts when brought in contact with tin, compared with the almost insensible influence which it has upon antimony, led M. Thenard to think that it might be employed as a means of separating these two metals when found in union; but the experiments which he made, with this object, were not attended with the success he anticipated.

Having procured tin and antimony in as pure a state as possible, I began by examining the action of hydrochloric acid on these two metals taken separately.

I flattened, to about the thickness of a centime, a portion of the tin, and put it into a phial, into which I afterwards introduced a certain quantity of hydrochloric acid perfectly pure, the specific weight of which was 1190, and which marked 22° on an areometer of Baumé's. At the end of six hours the dissolution was perfectly complete.

On applying a slight boiling heat to the same mixture, the dissolution was completed in one hour.

I then pounded 100 parts of antimony to a fine powder, and put them into a phial with a quantity of hydrochloric acid. After remaining twenty-four hours in a cold place, I found that 96 parts of the antimony remained at the bottom of the vessel, and that only four parts had dissolved†.

A like

* From the *Annales de Chimie et de Physique* for December 1816.

† After being some hours in the glass, the liquor which was at first white became yellowish. On shaking the glass this colour disappeared, but returned in a few hours after: the glass being again shaken, the colour again disappeared;—and as often as the same operation was repeated a like result followed.

A like mixture being set to boil, a similar proportion of four parts of antimony was found dissolved at the end of three hours.

The dissolution of these four parts in both cases appeared to be greatly owing to the presence of sulphuret of antimony in the antimony employed, a slight disengagement of sulphuretted hydrogen gas having been observed during the dissolution.

I next made two mixtures; one containing equal parts of tin and of antimony, and another 25 parts of antimony and 75 of tin.

The first of these mixtures, though harder than antimony, was so brittle that it broke on falling to the ground, and was very easily reduced to a fine powder. Its specific weight, estimated with much care, was found to be 6·803, the water distilled forming 1.

The second mixture, the specific weight of which was found to be 7·059, was first flattened with a hammer and then slightly chopped. It was easy to perceive that the tin had, with the smaller proportion of antimony here combined, already lost somewhat of its ductility.

The following are the experiments which I made on these mixtures:

First Experiment. One hundred parts of the first mixture, containing equal parts of tin and antimony, with an equal proportion reduced to a fine powder, were heated and treated with hydrochloric acid:—the action seemed very slight; the metal preserved its whiteness and its metallic brilliancy. After about three hours of ebullition I filtered the mixture, and found that 33 parts of the metal had been dissolved.

Second Experiment. One hundred parts of the second mixture, containing twenty-five parts of antimony and seventy-five of tin, after being beaten very thin, were heated and treated with hydrochloric acid. The action was much greater than in the preceding experiment; the metal from being perfectly white became slightly browned. After an hour of ebullition, no increase of action appearing, I filtered, and found that 53 parts of the metal had been dissolved.

From these two experiments it was easy to see that the antimony, besides being itself nearly insoluble in the hydrochloric acid, served to counteract the dissolution of the tin; and that in proportion as there was less of antimony in the mixture, there was more of the tin dissolved.

followed. This yellow colour is undoubtedly owing to a certain quantity of chlorine, formed at the expense of the oxygen of a part of the water decomposed, and which from its tendency to a gaseous state disengages itself on the agitation of the liquid.

On recalling to mind the process which is used to extract by means of nitric acid the silver contained in gold, and the necessity there is in that case of employing three parts of silver to one of gold — a process commonly known by the name of *quartation*,—it occurred to me that tin might be to antimony as silver is to gold, and that the proportion of tin necessary to the *quartation* of antimony ought to be much greater than what I had used in the preceding experiments,—that this only required to be sought after to be ascertained. I accordingly made the following experiment:

Third Experiment. I weighed with great care, in a balance capable of indicating the very nicest proportions, ten parts of antimony and 90 parts of tin, being as nine to one. I enveloped the whole in a piece of paper, placed it in the bottom of a small experimenting crucible, covered it with a layer of fine powder of charcoal, in order to prevent oxidation, and subjected it to the heat of an essay furnace for ten minutes. I then withdrew the crucible from the fire, and allowed it to cool. On examining it I found a small drossy mass, which I first brushed carefully, then flattened, and afterwards cut into a number of pieces. I wrapped up these anew in paper, and subjected them to the heat of the stove in the same manner as before. The mixture being again cooled and well brushed, was without being flattened fused a third time, and only a very small slip of paper interposed between it and the charcoal. The residue which I now obtained consisted of a small compact knob, of metallic brilliancy, and exactly 100 parts in weight. I flattened this as thin as I could, and cut it with scissars into small fragments, which I put into a phial and afterwards saturated with hydrochloric acid. Having boiled the whole for two hours, I allowed it to cool, and then collected upon a filter the portion not dissolved. I found it to be of the weight of twelve parts in place of ten.

Fourth Experiment. I joined with the same care as in the preceding experiment, five parts of antimony to 100 of tin, being as 1 to 20. The knob obtained as before, being flattened as thin as possible, cut into pieces, and these put into glass; I saturated them with hydrochloric acid. The action was quick, and at the end of a quarter of an hour the dissolution appeared almost complete. The antimony abstracted floated in the liquor: I made it still boil for two hours longer; and on then collecting the residue on a filter, I found just five parts of antimony—being the precise quantity which I had put into the mixture.

On repeating this experiment, I obtained exactly the same result.

I made

I made a similar one with 15 parts only of tin in place of 20 to one of antimony, and found a surplus.

In another experiment, with the tin in the proportion of 30 to one, the result gave me back exactly the proportion of antimony put into the mixture.

I last of all put only a centime, and afterwards the fourth of a centime of antimony to 100 parts of tin, and recovered them perfectly.

The antimony in these different experiments was extremely divided, of a blackish gray colour, settled easily at the bottom of the vessel, and occupied, in proportion to its weight, a very considerable space, so as to enable even imponderable quantities to be discerned.

It is after all very true, that antimony as well as tin is soluble in hydrochloric acid, and that its presence was not indiscernible in the hydrochlorate of tin produced in the fourth experiment. But this I think may be easily reconciled with the truth of the preceding results. The hydrochloric acid having a remarkable affinity to tin, would of course saturate it first, and thus lose almost entirely its power of acting on the antimony. It is hard to believe that any tin can remain in alliance with the antimony, when we find the different residues corresponding so exactly with the quantities of antimony introduced; and much more probable that the portion of antimony observable in the hydrochlorate of tin, may be a portion not appreciable by weight.

As it often happens that lead is joined to the tin already united with the antimony, I made an experiment in order to ascertain at what point this new metal might be able to change the results obtained by my preceding mode of analysis; and I perfectly satisfied myself that the mixture of tin and antimony may contain large enough quantities of lead without affecting the success of the operation.

XCIII. *On the Cultivation of the Parsnip, as it is practised in Guernsey. By Dr.* Macculloch, *of Woolwich**.

The great superiority of this root, as cultivated in Guernsey and the neighbouring islands, to its produce in Britain,—the high reputation which it bears among the farmers in those islands, and the very little knowledge of it, which those of Britain seem to possess,—have induced me to lay before the Society a short account of the methods practised in its culture in Guernsey.

I am inclined to think, that it will be found much more worthy the attention of agriculturists than has been hitherto supposed, and

* From the Transactions of the Caledonian Horticultural Society.

and that it will form a material and valuable addition to the system of green crops, when it shall become better known.

But it is chiefly on account of the power which it possesses, of resisting the injuries of frost, that I have ventured to point it out as an object of attention to the Caledonian Horticultural Society. The injury which the green crops, commonly cultivated in the northern parts of our island, suffer from this enemy, is such as to render it highly desirable to find one which shall be exempt from the effects of winter.

It has been hitherto but generally and carelessly said, and as if the fact was not well ascertained, that this root did not suffer from frost. The unusually severe winter of 1813–14 has enabled me to decide this question most positively; and to name the parsnip as perhaps the only cultivated root which appears to defy all cold. In the garden of my friend Mr. Mathews, at Waltham Abbey, a crop of parsnips was suffered to continue in the ground throughout the winter. That land is well known to be wet meadow land, and was frozen in a solid mass, to the depth of a foot or more. The roots remained unhurt; and while I write, in the beginning of April 1814, they are all putting out their new shoots. This hardiness, which would render the parsnip a desirable object of cultivation in the coldest parts of Scotland, would still more recommend its use to the unfortunate Greenlanders, among whom the esculent vegetables have hitherto been limited to two or three, and where the parsnip has not as yet been introduced. If other circumstances (the method of culture, the deep ploughing required, and the nature of the soil necessary for this root,) do not prevent its introduction into the Highlands, it may eventually be found a valuable substitute for the potatoe, in many situations where the early frosts often destroy that plant long before the tubers have arrived at maturity. It is no small additional merit, that it is nearly exempt from the attacks of insects; and from the diseases incident to all our esculent roots, as well as from the effects of cold. In wet springs only it is remarked, that the plants in Guernsey are sometimes destroyed by slugs, and that extremes of dryness or moisture protracted through the season are injurious to them.

The superior quality and size of the root in Guernsey appears to be the result of the long-continued care and attention bestowed on it, since there is nothing in the soil of that island to account for this difference; and since that soil itself is by no means of a very superior quality. The greater part of the island consists of a large foliated gneiss, impregnated with a considerable proportion of iron, and subject to decomposition in the mass by a process of rotting or gangrene similar to that which occurs in many varieties of the trap family, and among other places very remarkably

remarkably in Sky. The result is consequently a gravelly loam. It is remarked by Mr. Young, that the parsnip requires a rich putrid dry sandy loam; and he discourages its cultivation where the soil is not of this desirable quality. This is assuredly not the character of the soil of Guernsey, where the cultivation is successfully carried on, even in situations where the land is stiff, cold, and wet. An open and loose soil is obviously necessary, to allow of the growth and descent of the long-rooted variety; but it will be observed by and by, that two varieties are in use, of which the one is much shorter than the other; and consequently better adapted to soils of no great depth.

These two principal varieties, cultivated in Guernsey, are known by the name of the *Coquaine* and the *Lisbonaise*.

The first of these roots is the finest, and sometimes runs four feet deep. It is rarely so small in circumference as six inches, and has been known to reach sixteen. The leaves of this variety grow to a considerable height, and proceed from the whole crown of the root.

The *lisbonaise* does not extend to so great a depth as the *coquaine;* but the root is as good, and is preferred by many farmers, since that which is lost in length is gained in thickness, and it does not require so deep a soil. Though the crown is equally large in this variety, the leaves are small and short, and only proceed from its centre, in which there is a hollow or cup. The root terminates rather abruptly, in small fibrous radicles.

There is yet a third variety known by the name of *fourquée* (forked), which appears to be only a modification of the last, and which, although still cultivated, is less esteemed.

On comparing the dimensions of these roots with those of the variety cultivated in Britain, it will be seen that the former are much superior; and it is supposed that their qualities in Guernsey are also superior to those of our varieties. It will likewise be seen that they can produce a heavier crop in that island on the same extent of ground.

From these considerations, it would be advisable to cultivate the Guernsey varieties in this country, by procuring the seeds from that place, and to abandon those whose produce seems in every respect inferior.

Although this root is cultivated in almost all the soils of that island, that is esteemed the best which consists of a good light loam, the deeper the better. If the loamy soil is not deep, the under soil at least should be open, to allow of the free penetration of the roots.

If the land is not perfectly clear from couch-grass and other weeds, it is pared with the paring-plough in October, and harrowed to remove the weeds. About the middle of February, the

land is prepared for sowing by means of two ploughs. A small plough precedes, and opens the furrow to the depth of four inches, and is followed by a larger plough, drawn by four or six oxen and as many horses, which deepens the furrow to ten or fourteen inches. This plough is called the *grande charrue;* and as the small farms into which Guernsey is divided do not admit of such an army of cattle in the possession of any one farmer, this work is performed by a contribution of the neighbours, who are repaid by the like joint-stock assistance, the whole being attended with a holiday-like bustle, that cannot fail to surprise a stranger. I need not remark, that a more accurate system of husbandry could accomplish the subsequent trenching and turning up of the first furrow with much less force. The spade is used for this purpose in some parts of France as well as in Jersey, but is less expeditious and œconomical than a judicious use of the trenching-plough. As soon as the clods are capable of being broken, the harrowing commences, and is repeated till the soil is pulverized, and reduced nearly to the state of garden mould. The whole of these processes are intended to loosen the soil to as great a depth as possible.

The seed should not be more than a year old, as it is uncertain when of a greater age. It is sown broadcast, and in a day just so windy as to insure its regular spreading over the surface. The seed is then covered by the harrow. The quantity sown is from half a *denerel** to one *denerel* per *vergée*. The half *denerel* is judged sufficient; but many farmers sow the whole, to enable them to harrow the land before the first weeding, by which means they destroy so many weeds as to save much of the after hand-weeding. As soon as the plants are sufficiently strong, they are hand-weeded and thinned; and this operation must be repeated at least three times during the summer. The distance between the plants is ultimately about nine inches; and, to save a portion of the labour, a harrowing is sometimes given between the first and second weedings. The expense of weeding a *vergée* three times, is thirty shillings. I believe that the practice of drilling and horse-hoeing, by which much labour might probably be saved, has never been attempted in Guernsey, where agriculture has not arrived at that perfection which it has attained in this country, and where, from the infinitely small division of property, and consequent size of the farms, with the almost unavoidable attachment to ancient practices which accompanies these circumstances, its operations are in general antiquated.

* The *denerel* is four quarts; the *vergée* 17,640 square feet; 2·46 *vergées* are equal to an English acre, which consequently gives about ten quarts to the acre. The price of parsnip seed while I write, is 2*s.* 6*d.* the *denerel*, making the whole expense per acre nearly 7*s.* 6*d.*

It

It is indeed said, that in other countries, where this method has been tried, it has not been found to answer so well as the system of broadcast culture.

The first weeding is performed about the middle of May, or it may be earlier or later according to the state of the plants; it is repeated when necessary, till the beginning of July. The distance at which the plants are allowed to remain, is greater in Guernsey than in England, where they are suffered to stand at six inches asunder.

Although the general practice is that which I have now described, the seed is sometimes sown at the latter end of September or beginning of October, and the plants are found to pass the winter well, and produce a good crop. It is supposed that they may thus become strong before they can be injured by weeds. There is also some variation in the time of the spring sowing. Where the soil is a rich sandy and dry loam, the seed is sown early in January; but the general period of sowing over the whole island, is from the middle of February to the beginning of March, except in stiff and wet lands, where it is deferred for a fortnight.

The produce per acre is considerably greater than that of the carrot. A good crop in Guernsey is considered 17,600 lbs. per *vergée*, or about 44,000 lbs. per English acre. This is a less heavy crop than the turnip, but it is much more considerable than that either of the carrot or potatoe. If we consider at the same time that the quantity of saccharine, mucilaginous, and, generally speaking, of nutritious matter in the parsnip, bears a far larger proportion to the water, than it does in the turnip, its superiority in point of produce will appear in this case also to be greater.

The roots are dug up about the middle of August, when they are thought to be most nutritious, and to fatten animals better than after the leaves are decayed. I do not understand that the green tops are used in Guernsey, although in England they have been found as useful for live stock, as other green food, either consumed in the field, or cut off when the roots are taken up. The quantity dug up at this season is not more than is required for two or three days consumption. It is only in October that the root is fully ripe, when it may be dug up with forks, and preserved dry in sheds during the winter; but it is usually left in the ground in Guernsey, where frost is rare, and taken up as it is wanted.

The parsnip is considered by the farmers of Guernsey as the best fallow crop known, and as in the greatest degree influencing the subsequent crop of grain. In Jersey, it is the usual practice to follow it by wheat. As it draws its nourishment from the

deeper

deeper parts of the soil, it is evident that it is particularly calculated to succeed the generality of fibrous-rooted vegetables. If sown, therefore, after a hay or barley crop, it seldom needs any manure, and yields a very good produce without it. In England, where manure is required, farm-yard dung is preferred, and it is turned into the soil by a light plough, immediately before sowing the seed. But in Guernsey, sea-weed is universally adopted when it can be obtained—a species of manure in which many districts of the Highlands abound, although its use is by no means so extensive as it deserves to be. The recent and apparently steady diminution in the price of kelp now going on, will doubtless introduce this valuable manure into much greater use in the Highlands, than has hitherto been the case.

The parsnip is considered by the Guernsey farmers to be the most nutritious root known, superior even to the carrot and the potatoe. When small, it is given to the animals whole; but when large, it is sliced longitudinally. As no farmer in Guernsey feeds his horses or cattle on parsnips alone, it is not possible to determine its exact value from their practice, with the accuracy which the more scientific agriculturists of this country would desire. The art has not yet attained in that island the same precision, nor been subjected to the same laws of rigid calculation, which it has undergone in Britain. But a tolerable conclusion may be drawn of the efficiency of this root, even from the examination of the testimony of Guernsey farmers.

Cows fed with parsnips are said not to yield so great a quantity of milk as when fed with turnips: but the milk is richer, and the butter is better, as well as in far greater proportion; and both are also free from the disagreeable flavour which they acquire from turnips; a circumstance highly deserving the attention of those dairy farmers who supply the population of great towns with these indispensable articles of consumption. They are in fact equal to those which are produced by feeding in the best pastures. These animals, when intended for the butcher, are observed to fatten faster and better on parsnips than on any other food. The only precaution used, is to interpose hay, to prevent them from being surfeited with the root. It is also found necessary to begin with a smaller proportion, as they are apt to be satiated with this food in the first week, if given to excess. After that period, it is remarked that it may be used in any quantity.

The farmers are of opinion that cabbages are the best substance to interpose for this purpose, although turnips or hay may also be given with the parsnip. The allowance for fattening an ox who will weigh 1100 lbs. is 120 lbs. per day, exclusive of hay. As far as any experiments have been made in England, the results

sults tally with those here reported. The cattle were found to fatten quicker, and become more bulky, than when fed with any other root; and the meat has also turned out more sweet and delicate. In some experiments, recorded as having been made by an experienced farmer in Surrey, an ox was fattened from the plough on parsnips alone in thirteen weeks. I may add, that in many parts of France, and among the rest in Brittany, where this root is extensively cultivated, the same results have been obtained. Beef fattened with parsnips fetches a half-penny per pound more in Jersey than under any other system of fattening.

Hogs prefer this root to all others, and make excellent pork; but it is fancied in Guernsey, that the boiling of the root renders the bacon flabby. It has, however, been found in the trials of the Surrey farmer above mentioned, that the hogs became satiated with the raw parsnips before they were fattened, upon which he caused them to be boiled, with good effect. The animal can be fattened in six weeks by this food.

Horses are equally fond of the parsnip, although, from eating it with too much avidity, it is said sometimes to stick in the throat, and to choke them. But this may be easily prevented by cutting the roots into pieces before they are given. The use of parsnips is said to affect the eyes of this animal; but we may safely consider this assertion as somewhat apocryphal. They are found to supersede the necessity of corn, except when the work is excessive; and in Brittany they are even used for this purpose, to the exclusion of corn.

I may add, that it is a popular opinion among the Jersey farmers, that all animals intended for the butcher may be fattened on parsnips in nearly half the time, and with half the quantity which is required in feeding them with potatoes. This must, however, be taken rather as a general opinion, with regard to the superiority of the one root over the other, than as the result of any accurate set of experiments; since the practices of agriculture in that island, as well as in Guernsey, are by no means reduced to that nicety of calculation which they have hitherto experienced in Britain.

In Brittany they also form a principal article of the food of the people, and are still used largely, notwithstanding the introduction of the potatoe; but I need scarcely add, that, as in the case of most other roots, the potatoe has to a great degree also diminished the consumption of parsnips as an article of human food. The peculiarity of their flavour is such, as perhaps for ever to prevent them from entering into competition with that most valuable plant; although in situations similar to the highland districts to which I have above alluded, the cultivation of the parsnip

parsnip, to a certain extent, might probably be found a useful resource, at least as an auxiliary article of food, in case of the failure of the potatoe.

Before terminating this paper, I may remark, that a species of wine has been often manufactured from the fermented juice of parsnips, and that report speaks in its favour. I have no experience of it; and for obvious reasons, there has never been any temptation in Guernsey or its neighbour islands to discover substitutes for the untaxed and superior produce of the vine. Nor do I know that the parsnip wash has been subjected to distillation. It would be worthy the attention of the Society to inquire, whether the spirit produced from it might not become a substitute for whisky, since the produce per acre would unquestionably be much greater.

I may also add, that parsnips are cultivated to great extent in Jersey as well as in Guernsey, and with the same favourable results, though with some little variation in the process.

The preparation of the land there, and the other previous arrangements, are similar to those already described. After the harrow, the ground is dibbled with beans, in rows at five feet distance. The parsnip-seed is then sowed over the whole, broadcast. In May the hand-weeding commences, and the parsnips are thinned to the requisite distance. The beans are pulled up by hand in September, and the parsnip crop is then disposed of as in Guernsey. I have not been able to procure any accurate estimate of the comparative value of the two processes, nor to learn how far the bulk of the parsnip crop is diminished by the additional incumbrance imposed on the land by the beans.

XCIV. *A Method of cultivating Asparagus, as it is practised in France. By Dr.* Macculloch *.

That part of the garden which is longest exposed to the sun, and least shaded by shrubs and trees, is to be chosen for the situation of the asparagus quarter. A pit is then to be dug five feet in depth, and the mould which is taken from it must be sifted, taking care to reject all stones, even as low in size as a filberd nut. The best parts of the mould must then be laid aside for making up the beds.

The materials of the bed are then to be laid in the following proportions and order:

Six inches of common dunghill manure.
Eight inches of turf.
Six inches of dung as before.

* From the Transactions of the Caledonian Horticultural Society.

Six

Six inches of sifted earth.
Eight inches of turf.
Six inches of very rotten dung.
Eight inches of the best earth.

The last layer of earth must then be well mixed with the last of dung.

The quarter must now be divided into beds five feet wide, by paths constructed of turf, two feet in breadth, and one foot in thickness. The asparagus must be planted about the end of March, eighteen inches asunder. In planting them, the bud, or top of the shoot, is to be placed at the depth of an inch and a half in the ground, while the roots must be spread out as wide as possible, in the form of an umbrella. A small bit of stick must be placed as a mark at each plant, as it is laid in the ground. As soon as the earth is settled and dry, a spadeful of fine sand is to be thrown on each plant, in the form of a molehill. If the asparagus plants should have begun to shoot before their transplantation, the young shoots should be cut off, and the planting will, with these precautions, be equally successful, though it should be performed in this country even as late as July. Should any of the plants originally inserted have died, they also may be replaced at this season. The plants ought to be two years old when they are transplanted; they will even take at three; but at four they are apt to fail.

If it be necessary to buy asparagus plants for these beds, it will be proper to procure twice as many as are required. The best must then be selected for planting, and the remainder placed in some remote portion of the prepared bed, or into a similar situation, but without separating the plants. Here they must first be covered with four inches of sand during the summer, and as soon as the frost sets in, with six inches of dung over that.

The stems of the planted asparagus must be cut down as soon as the frost commences, and close to the ground. The beds are then to be covered with six inches of dung, and four of sand. In March, the bed must be stirred with a fork, taking care not to approach so near to the plants as to derange them. Towards the end of April, the plants which have died, may be replaced with the reserved ones lately described.

In three years, the largest plants will be fit to cut for use. If the beds be sufficiently large to furnish a supply in this manner, the asparagus shoots should be cut as fast as they appear; otherwise they must be left till the quantity required has pushed forth; in which case the variety in colour and size prevents them from having so agreeable an appearance. An iron knife is used for this purpose.

In cutting, the knife is to be slipped along the stem, till it

reaches

reaches the bottom of the shoot, where the cut is to be made. At the end of four years the great and small ones may be taken indiscriminately. The cutting should cease about the end of June.

At the beginning of winter the stems are all to be cut away, and the beds covered with dung and sand in the manner above described. If muddy sand from the sea-shore can be procured for the several purposes above described, it is the best; otherwise, river sand may be used; and if that cannot be procured, fine earth must be substituted.

The asparagus bed now described will generally last thirty years; but if they be planted in such abundance as to require cutting only once in two years, half the bed being always in a state of reservation, it will last a century or more. The turf used in making the beds should be very free from stones.

Care must be taken not to tread on the beds, so as to condense the earth in planting the asparagus; and to prevent such an accident happening on any other occasion, a plank should be used to tread on. It must be remembered, that the division of the beds, which is formed by thick turf, is intended to prevent the condensation of the earth below, in consequence of the necessary walking among the beds. As in the course of time this condensation will gradually take place, the turf ought to be renewed every three years, for the purpose of stirring the ground below: and in applying the winter coat of manure, it must be remembered, that even these walks are to be covered. If these circumstances are not attended to, or if the earth below the walks has not originallly been constructed in the way described above, the asparagus plants which grow near the walks will be much less fine than those in the middle of the beds.

*** I understand that this plan has been put in practice by Mr. Allan, of Tweedside, with success.

XCV. *Observations upon the Alveus or General Bed of the German Ocean and British Channel, and on the Encroachments of the Sea on the Land. By Mr.* Robert Stevenson, *Civil Engineer, Edinburgh.*

Observations upon the wasting of the land by the encroachment of the sea might, with great propriety, be made upon the shores of Ireland, of which I have seen many instances on the western, northern, and eastern coasts, from Loch Swilly in the county of Donegal, to the Tusker rock off the coast of Wexford. But, without enlarging upon these shores, we shall now turn our attention

attention to the coast of England, which, with the opposite shores of Holland and France, form the apices of the German Ocean and the British Channel. From the more soft and yielding matters of which these shores are formed, particularly those of England, which are at the same time exposed to the violent attacks of the sea in storms from the north-east and south-western directions, the wasting effects of the sea are altogether so very remarkable, that it may in general be affirmed that these shores are in a progressive state of waste. Beginning with the north-eastern coast, examples of this will suggest themselves to the recollection of those who are acquainted with the shores of Northumberland, Durham, and Yorkshire; as at Holy Island, for example, and the shores near Bamborough Castle, where the sea has made considerable inroads upon the land, in the recollection of the present inhabitants of that neighbourhood. Tynemouth Castle, situated at the entrance of the river Tyne, which now in a manner overhangs the sea, had formerly a considerable extent of land beyond it. Tynemouth head, being composed of a soft sand-stone, is gradually worn away by the action of the sea and the effects of the weather; and every season falls down in such quantities, that the degradation is quite observable to the inhabitants of the town of Tynemouth. Upon the southern side of the entrance to the river Tyne many acres of land have been washed away from the extensive ebb called the Middens; and the same has happened along the whole shores of the county of Durham, particularly between the rivers Tyne and Weir, where the coast is chiefly composed of a soft friable limestone: and indeed the land is obviously in a state of waste all the way to the Tees. On the southern side of the great sand-banks, forming the mouth of the Tees, we enter upon the coast of Yorkshire, which extends to the estuary of the river Humber, being upwards of a hundred miles. This coast consists chiefly of sandstone and chalk-hills, and exposes a precipitous face to the sea, which is acting upon it, and in many places producing its rapid destruction:—of this many examples are familiar to those on the spot, particularly in the neighbourhood of Whitby and Scarborough. For a few miles both on the northern and southern side of Flamborough-head lighthouse, the section of the coast is almost perpendicular, and consists of chalk intermixed with portions of clay. At the eastern extremity, or pitch of the head, the chalky cliff is about seventy feet in height: from this point the coast declines all the way to the town of Bridlington, and from thence to Dimbleington cliff, near the entrance to the Humber, it is a low sandy shore. From what has been already stated of the effects of the sea upon the hard or more compact shores of Scotland, it is easy to imagine what its operation must be

be on the line of coast just described: accordingly, the inhabitants at Flamborough-head, and indeed all along the Yorkshire coast, are too often kept in mind of this by the removal of their landmarks and inclosures; and there are many traditions of churches, houses, and whole fields having been overrun by the sea in the neighbourhood of Hornsea, Kilnsea, and the Spurnpoint light-houses on the northern side of the Humber. The widely extended mouth of this estuary, and the manner in which it is cumbered with sand-banks off the coast at Clea and Saltfleet in Lincolnshire, and indeed the appearance of the coast all the way to Boston, shows that much of the land has been swallowed up or overrun by the sea; of which there are many striking proofs, both of ancient and modern occurrence.

The same remarks are also applicable to the great ebb, called *the Wash*, forming the entrance or navigation to the harbours of Boston and Lynn. Here it would appear that the sea has made a breach through the chalk hills, which are observable on each side of the Wash, in the counties of Lincoln and Norfolk, where it is obvious that the land has at one time extended further into the sea, and is at present undergoing the process of actual waste. Perhaps evidence of this may also be drawn from the works of William of Malmesbury, who represents the whole of the fens of Lincoln to have been in a state of high cultivation in the eleventh century. But certainly a most unequivocal proof of this is afforded from the discovery of Sir Joseph Banks and Dr. Joseph Correa, mentioned in the 89th volume of the Philosophical Transactions, of the remains of a sub-marine forest on this coast, now several fathoms under water, where the roots, boles, and branches of trees, particularly of the birch, of large size, were discovered. From the account of the fishermen of this coast, these appearances are to be seen for many miles along the shore in the form of a range of small islets: and trees have been often found, the timber of which was so fresh as to be fit for œconomical purposes. The inhabitants of the country likewise represent, that at one time the parish-church stood greatly within the present sea-mark, and that the walls of houses, of a former village, have been seen at low ebbs; and they allege, that even the clock of the present parish church, is the same that was in the church the foundations of which are now overflowed. It seems therefore probable, that the present state of the fen country arises from the encroachments of the sea, occasioned by the silting or filling up to a certain degree of the alveus or bed of the German ocean, rather than from the gradual retreat or subsiding of the waters of the ocean; and that the sea, notwithstanding some anomalous instances of recession which shall afterwards be noticed, is invariably trenching upon the land.

Proceeding

Proceeding southward, we next traverse the coast of Suffolk and Essex, where numerous instances occur of ravages which the sea is making upon the shore. It has already been ascertained, that the sand-banks of Yarmouth Roads have of late years considerably altered, and that the depth of water is perhaps upon the whole rather lessened; and some pretty extensive additions have been made to the land at the junction of the rivers Alde and Butley in the great gravelly beach which extends about eight or ten miles in length, varying in breadth from a few hundred feet to about a mile; and similar appearances are to be found on this coast as at Harwich, near the confluence of the rivers Stour and Ipswich, where a considerable addition has been made to the land on the southern side of Landguardfort: yet these, and other examples of the same kind, are trifling in proportion to the astonishing effects of the sea in destroying the land in this very neighbourhood. Near Leostoffe, Dunwich, and Aldborough castle, on the Suffolk coast, the sea is daily making impressions upon the land, which is apparent to the observation of every one acquainted in the slightest degree with that coast, and is at some places severely felt both by the proprietor and the tenant. At the Naze Tower, near Walton, and indeed all along the coast of Essex, the same appearances are no less obvious.

Crossing the numerous sand-banks and shoals which greatly encumber the mouth of the river Thames to the Kentish coast, we are every where presented with instances of the degradation of the land by the encroachment of the sea. From Sheerness along the shore of the isle of Sheppy, and from the entrance of the river Swale to Margate and Ramsgate, at various places, very large portions of the chalky cliffs are continually giving way to the sea. At Sheppy island, Thanet and Sandwich, there are proofs of the land gaining somewhat upon the sea: of this the Goodwin and other sand-banks may also be considered as examples. But these cases, arising from the shape of the coast, and the set of particular currents of the tide, are evidences of the silting up of the alveus or bed of the ocean, and shall be afterwards alluded to as so many proofs of the consequent tendency of the sea to overflow its banks. But, to continue, it may further be noticed, that the streets of Deal are often laid under water; and houses there have occasionally been washed down by the sea: and, indeed, its effects are very alarming all along this coast.

At Romney Marshes labourers are constantly employed attending and repairing the fences and sea-dikes of these low shores. On the precipitous shores from Deal to Dover, Folkstone and Hithe, large portions of the chalk-cliffs are frequently undermined and carried away; particularly at the South Foreland and cliffs of Dover, where I happened to witness the effects of the recent fall,

 some

some years ago, of an immense quantity of these extraordinary chalk cliffs, the ruins of which appeared to cover several acres of ground, and must have contained many thousands of tons. A fall of this kind, near Beachyhead, on the Sussex coast, is noticed in a paper by Mr. Webster in the Transactions of the Geological Society. The portion which gave way extended 300 feet in length, and was 70 or 80 feet in breadth. A clergyman who happened at the moment to be walking on the spot, observing the ground giving way, had just time to escape when the whole fell down with a dreadful crash. In the same manner the opposite coast of France is understood to be acted upon; and the numerous islands lying off that coast and the coasts of Germany and Holland. I might also extend these observations to the shores of Hampshire, Dorset, Devon, Cornwall, particularly to the isles of Wight and Portland, and the Scilly islands; the wasting of the land and the encroachment of the sea being everywhere remarkable, and always in proportion to the nature of the strata or rocks composing the coast, whether alluvial, chalk, limestone, sandstone, or granite.

Nor are these effects of the sea confined to the shores of the German Ocean and the British Channel; for the wasting of the land is no less remarkable in St. George's Channel and the Irish Sea, including the coast of Ireland on the one side, and, on the other, the shores of Wales, Lancashire, Westmoreland, and the counties of Dumfries, Kirkcudbright, and Galloway, where neither the rocky coasts, and exposed situations of the islands of Anglesea, Man, Copland, Craig of Ailsa, and the islands of Cumbrae, nor the sheltered and alluvial shores of the Bristol Channel, are exempted: even the indentations of the coast at Dublin Bay, Liverpool, and Lancaster, and the more extensive friths of the Solway and the Clyde, are subject to the unvarying destructive effects of the sea.

Having pointed out, from actual observation on about one-half of the coast of Ireland, and on all parts of the shores of Great Britain, from the Scilly islands, its southern extremity, to the Naze of Unst, or northernmost point of Shetland, that the land, on the margin of the sheltered bays and friths of our coast, as well as on the most exposed promontories and open shores, is undergoing the process of waste and decay from the impulse and action of the sea, I shall in a future paper endeavour to show, that the cause of this effect, particularly on the shores of the German Ocean and British Channel, is, in a good measure, owing to the immense quantity of debris which must be accumulating, at least to a certain depth, in the bottom of the ocean.

This paper is circulated, with a view of obtaining additional facts regarding the wasting of the shores of Great Britain and

those

those of the opposite continent; and more especially to procure intelligence respecting the numerous examples of the formation of new land and banks under water from the deposition of gravel, sand, and alluvial matters, at the mouths of rivers, in bays and creeks along the shores, or in the open sea.

XCVI. *Mr.* Pye's *Method of preparing Charcoal and Chalks for Drawing*.*

The finest-grained charcoal that can be procured is sawed into slips of the size and form required, and put into a pipkin of melted bees'-wax, where they are permitted to remain near a slow fire for half an hour or more, in proportion to the thickness of the charcoal: they are then taken out, and, when perfectly cool, are fit for use. By adding a small quantity of rosin to the wax, they may be made considerably harder; and on the contrary, should they be required softer, a little butter or tallow will answer the purpose. The advantages these pencils possess are, that they can be made at the most trifling expense, and at any time; and that drawings made with them are as permanent as ink, and not liable to injury by being rubbed, or remaining in the damp.

The above process will harden both red and black chalks, and make them permanent also.

XCVII. *On the Effects of Galvanism in Asthma and Diseases of the respiratory Organs. By* A Correspondent.

The successful experiments of Dr. W. Philip and the Worcester practitioners should have attracted more attention from medical inquirers than they appear to have hitherto done. The phænomena of galvanism are yet very imperfectly known; and many may still be discovered by multiplying experiments in this department of science, and particularly its application to the animal œconomy. It is, however, a general propensity of the day to expect a complete and perfect renovation of a vitiated constitution merely by the application of some one potent drug; —an expectation, perhaps, not less injurious to the progress of science (so far as it is concerned with medical practice) than the obsolete notions of blending together almost all the known drugs into a single dose. Hence, it is probable, that as the application of galvanism to the respiratory organs has not wrought any mi-

* From *Transactions of the Society for the Encouragement of Arts, Manufactures and Commerce,* for the year 1816.—The silver Isis Medal was voted by the Society to Mr. Pye for this communication.

 racles,

racles, nor instantaneously either killed or cured, its use has been almost entirely neglected. Perhaps, also, a certain degree of reluctance in patients to bear its action, may have contributed to retard its general application. It must, indeed, be confessed that it requires some patience and resolution to bear the smarting sensations occasioned by the application of the Galvanic wires to the nape of the neck and the chest. A few minutes of the weakest battery and conducting wires armed with silver knobs are generally sufficient to blister the parts to which they are directly applied. This effect takes place even when the skin is shielded by pieces of tinfoil, and the consequent smarting pain sometimes intimidates debilitated patients. In persons of gross habits, these blisters have been known to remain open for several weeks after they had been galvanized, and have ultimately proved highly salutary to the general health of the patient; in others, they healed as rapidly as common blisters. But it does not appear that any effects prejudicial to the constitution have yet followed the administration of galvanism; and it is certain that in many diseases it has a tendency to exhilarate patients, and might be very advantageously applied to melancholy persons. Out of five asthmatic persons galvanized by the writer of this, four have experienced very considerable relief; one has passed above nine months without experiencing any painful or even irksome effects in breathing; three others are attacked only in consequence of exposing themselves to sudden and violent transitions of temperature, but now in a much less degree than formerly; and another seemed to acquire additional energy and spirits, but very little melioration in his breathing.

In the course of these experiments some curious facts were observed. It was found that all the patients suffered more from the application of the wires when only half an inch or an inch of the Galvanic trough was covered with the acidulated water, than when the whole trough, consisting of two or of four inch plates, was filled. Very few persons could during the first minute bear more than six two-inch plates when less than half covered with the diluted acid; but when each plate was moistened to nearly its edge, then they could generally suffer four times the number. This apparently contradictory effect may be easily explained, by considering the necessity of atmospheric air to every Galvanic operation. If a battery composed of fixed plates be entirely covered with acidulated water, it has no Galvanic action; but whenever the atmosphere is allowed to act on the moistened metal, then its powers develop themselves. In like manner, when only the bottom part of the trough was moistened, and the communication made by rapidly pouring in a slender stream the acidulated water along the top of the battery, a greater portion

tion of the plates was exposed to the atmosphere, and consequently their action was too powerful for patients to endure. This fact may perhaps suggest the propriety of constructing Galvanic batteries in a manner not hitherto attempted, if not for chemical at least for medical purposes. It is indeed unquestionable that we are still unacquainted with many of the operations and powers of galvanism, and their relations to electrical phænomena; and a modification of the apparatus so as to present a greater surface to the atmosphere, in the same manner as it is necessary to connect an electrical machine with the earth, may perhaps contribute to augment the powers of this extraordinary implement of philosophical research. From a few experiments, which however were not sufficiently numerous to warrant a legitimate deduction, it appears probable that broad plates might be used to galvanize patients without blistering the parts so rapidly or exciting so much pain.

A. B.

XCVIII. *Geological Inquiries, proposed by the Geological Society.*

Introduction.

Geology relates to the knowledge of the system of our earth, of the arrangements of its solid, fluid, and aëriform parts, their mutual agencies, and the laws of their changes.

In this point of view, it is necessarily connected with many branches of Natural Science, but it is more particularly dependent upon Mineralogy, which distinguishes the species of inorganic bodies; and upon Chemistry, which investigates the intimate nature of matter and its hidden properties.

Geology in its comprehensive sense is consequently a sublime and difficult science; but fortunately for its progress it is susceptible of division into many different departments, several of which are capable of being extended by mere observation.

The knowledge of the general and grand arrangements of Nature must be collected from a number of particular and minute instances, and on this ground the slightest information relating to the structure of the earth is to be regarded as of some importance.—To reduce Geology to a system demands a total devotion of time, and an acquaintance with almost every branch of experimental and general Science, and can be performed only by philosophers; but the facts necessary to this great end may be collected without much labour, and by persons attached to various pursuits and occupations; the principal requisites being minute observation and faithful record.—The Miner, the Quarrier,

 the

the Surveyor, the Engineer, the Collier, the Iron Master, and even the Traveller in search of general information, have all opportunities of making Geological observations; and whether these relate to the metallic productions, the rocks, the strata, the coal of any district; or to the appearances and forms of mountains, the directions of rivers, and the nature of lakes and waters, they are worthy of being noticed.

It is with a view to facilitate and in some measure to direct general research, that the members of the Geological Society have collected from different sources and put together the annexed inquiries; and, as insulated remarks and local information can be of no avail, unless preserved and arranged, they venture to offer a repository for any facts that may be communicated to them. One great end of their association is to afford means by which this kind of knowledge may be concentred; and they conceive that by the labours and talents of many individuals thus united and assisted, several important objects may be easily attained; that Mineralogical maps of districts, which are now so much wanting, may be supplied; that the nomenclature of the science may be gradually amended by the selection of the most current and significant terms; that theoretical opinions may be compared with the appearances of Nature, and above all, a fund of practical information obtained applicable to purposes of public improvement and utility.—They address themselves more especially to their countrymen, and they cannot conclude without noticing the extraordinary facility of obtaining Geological information, afforded by the territory of the British Isles, and the peculiar interest which ought to be attached by their inhabitants to such inquiries. In no equal space is so great a surface of the earth laid bare by Nature and by art; no country is richer in those mineral productions on which some of the most important of our manufactures, and the most useful of the arts of life depend; and the present time is one in which we are particularly called upon to explore and employ the whole of our native riches and internal resources.

§ I. *Concerning Mountains and Hills.*

1. Are they solitary, or in groups, or do they form a chain?

If Solitary,

2. The general figure, as conical, pyramidal, &c.—more particularly of the summits?

3. The height above their base, and above the level of the sea?

4. The length, breadth, and general form of a horizontal section

tion passing through the base, or the *ground plan*; and the points of the compass between which the long diameter lies?

5. The degree of declivity on every side with regard to the circumjacent plain?

6. Do they present on any side abrupt craggy faces, and to what points of the compass are these opposed?

7. Do these precipices extend to the foot of the mountain, or are there at their bottom sloping banks of loose fragments?

8. Is the surface smooth or rugged?
—dry or marshy?

9. To what height does vegetation ascend, and what are the prevailing plants in different parts of the ascent*?

10. The springs, streams, lakes, hollows, gullies, caverns?

11. Whether any loose blocks of stone are found on the surface, different from those of which the mountain is composed?

In addition to the preceding Inquiries.
If in a Group,

12. Are the component mountains of nearly the same height?
13. Which are highest, the central or external ones?

If in a Chain,

14. The outline of the chain?
15. Its highest point?
16. Its length?
17. Whether straight or curved, and extended between what points of the compass?
18. Whether any lateral ridges proceed from the main chain?

§ II. *Concerning Valleys.*

1. Their geographical boundaries?
2. Their length, breadth, depth †?
3. Are they occasionally dilated and contracted, or do their sides preserve an uniform parallelism?
4. Is the bottom or floor even or rugged?—nearly level or much inclined? If inclined, whether regularly or interruptedly, and in what direction?
5. Are the slopes that form their sides smooth and gentle, or rugged and precipitous?
6. Do the opposite sides consist of the same kind of rock, and do they correspond in the inclination of their beds or strata?
7. Are there on their sides depositions of water-worn and rounded pebbles, either loose or compacted, and to what height do they reach?

* What are the variations in atmospheric temperature and moisture at the base, middle, and summit?—Edit.

† Their temperature, moisture, and prevailing winds?—Edit.

8. Are the detached fragments, by which the bottom is overspread, angular or rounded? of the same species of rock as composes the sides of the valley, or different?

9. Of what description is the solid rock or base upon which these rest?

10. Are they open or closed at one or both extremities?

11. Do any subordinate lateral valleys open into the main one, and what remarkable circumstances occur at their junction?

12. Do streams rise in or flow through them, and in what direction?

§ III. *Concerning Plains.*

1. Their shape and extent, with the nature, height, and general appearance of the hills or mountains by which they may be bounded?

2. The degree and direction of their inclination or slope?

3. The nature and character of the different soils by which they are covered?

4. Whether dry, or abounding in springs and standing waters?

5. If traversed by streams, in what direction do they flow?

6. Are the beds of rounded pebbles (if such occur) composed of minerals similar to those which form the surrounding mountains?

7. Have any opportunities presented themselves, in sinking shafts or wells, cutting canals, excavating docks and quarries, and digging foundations, of examining the subjacent strata, and what are the results of such observations?

§ IV. *Concerning Rivers.*

1. Their source*, their mouth?

2. The direction and length of their course, and whether these are the same now as formerly?

3. Their breadth, depth, and rapidity?

4. What is the rate of their descent or fall? is it uniform or interrupted?

5. The amount of their periodical increase or decrease?

6. The colour, temperature, and other properties of the water?

7. Whether any part of their course is subterranean?

8. Do they run in the same direction as the strata, or cross them, and at what angle?

9. The nature of the bed, whether rock, mud, sand, or gravel? Are the pebbles of the same rock as that of the adjacent country?

* Their temperature at different places?—Edit.

§ V. *Con-*

§ V. *Concerning Lakes, Springs, and Wells.*

1.—*Lakes.*

1. The extent, depth, temperature, and other properties of the water ?

2. The periods and amount of their greatest annual increase and decrease ?

3. Whether supplied by springs or streams, and whether any streams flow out of them ?

4. Of what is the bason composed ?

5. Are there any appearances that indicate the extent to have been formerly different from what it is at present; and does this alteration seem to have been gradual or sudden ?

6. Are there shoals of gravel and low islands in those parts where streams flow in; and do these increase from year to year ?

2.—*Springs.*

7. The physical and chemical properties of the water—the nature of its deposit ?

8. The quantity discharged in a given time, and the degree to which this is affected by dry or wet seasons ?

9. The kind of rock from which the water issues ?

3.—*Wells.*

10. Their depth ?

11. The number, thickness, and species of strata pierced through in sinking, and the order of their position ?

12. Whether all the wells of a district derive their water from the same stratum ?

13. Whether, when the water first flows, it rises rapidly, and accompanied by sand ?

14. Is the water liable to periodical increase or decrease ?

§ VI. *Concerning Shores or Coasts.*

1. If the shore is flat, to what extent ? and whence are the sand and pebbles derived ? Are they part of the adjacent cliffs, or brought down by rivers, or deposited by the sea ? in what quantity and of what description ?

2. If the coast is precipitous, the form and elevation of the cliffs, with the nature and disposition of the rocks which compose them ?

§ VII. *Concerning the Sea.*

1. Its depth, tides, currents, inlets, nature of the bottom, &c. ?

2. The height to which it rises ?

3. What effects has it produced on the adjacent rocks, &c. ?

4. Are

4. Are there any indications of its having formerly had a different level?

§ VIII. *Concerning Rocks.*

1. Their horizontal outline?

2. Are they separated from each other by thin bands of clay, or other extraneous substances? or slightly joined to one another? or firmly welded together?

3. When two rocks of different species come in contact, is any difference in colour, hardness, &c. observable between the adjacent surfaces and other portions of the same rock?

4. When a rock terminates at the surface of the earth, are any fragments of it to be traced in the form of gravel, &c.?—Does it re-appear after such interruption, and what is the nature of the intervening substance?

5. The form of their broken ends?

6. Are any rocks observed to terminate constantly together? and what are they?

If Stratified.

7. Is the stratification distinct or indistinct?

8. What is the number and thickness of the strata, and the order of their position?

9. Do they alternate or recur at regular intervals?

10. Do they, whether straight or waved, preserve their parallelism throughout, or are they cuneiform, &c.?

11. When vertical, what points of the compass are opposed to their sides, and what to their edges?

12. What is the amount of their dip, or the angle which they form with the horizon? and is it the same throughout their whole extent?

13. To what point of the compass do they decline?

14. Where several strata, of the same species, are incumbent on each other, do they differ in thickness or consolidation?

15. Where veins, dykes, or fissures occur, are the strata depressed, elevated, contorted, or altered in any other way?

16. How far does the external form of the mountain correspond with the position of the strata?

17. If the stratum contains broad and thin distinct particles, (such as mica) do these all lie in the same direction?

Note.—Care must be taken in examining strata, not to be deceived by distance or perspective, or by mistaking fissures for stratification, and fallen strata for strata in their natural position; and it should be kept in mind, that before the inclination of a stratum can be determined with certainty, it is necessary that it should be seen on two of its adjacent sides.

If

If Unstratified.

18. Are they amorphous, columnar, or in globular concretions?

19. Do they split with the same ease in all directions, or have they what is called a grain?

20. Do they abound in fissures, and what is the direction and extent of these?

§ IX. *Concerning the Materials of Rocks.*

1. Are they composed of one mineral substance, or of more? In the latter case, which has impressed the other?

2. Are they composed of parts cemented together, or adhering to each other without a cement?

3. Are they granular, slaty, porphyritic, amygdaloidal, or any compound of these? If breccia, are the included nodules large or small, entire or broken, &c.?

4. Do they contain fragments of other rocks, and of what description? Sand? Shells? Corals? Vegetable impressions, or any thing that appears to belong to a different formation?

5. Are there hollow nodules, and in what manner are they lined?

6. Is there any character, by which substances found in one stratum can be distinguished from similar substances found in another? or by which, what have been called primary strata may be distinguished from secondary strata, and strata of transition?

7. What minerals are found to be generally concomitants of others?

8. How are the several species affected by the combined action of air and moisture? Where large fragments have been torn by torrents from known rocks, what is the progress of their decomposition, and is there any re-aggregation?

9. What are the characteristic forms of each species of rock
—in mountains?
—in detached blocks?

10. How are they affected by peat moss lying on them?

11. What are the plants, the presence or absence of which indicates the nature of the soil?

12. By what local denominations are the different rocks distinguished, and to what œconomical purposes are they applied?

§ X. *Concerning Veins.*

1. Are they of the same materials as the rock in which they occur, or of any contiguous rock?

2. What is their direction with regard to the points of the compass, and the inclination of the adjacent strata?

3. Are they vertical, horizontal, or inclined, and at what angle?

4. What

4. What are their several dimensions?

5. Are they nearly of the same thickness at different depths? Do they terminate in a wedge; and this, at the top or bottom of the vein?

6. Is their longitudinal course straight or curved?

7. Is it of uniform breadth, or does it enlarge and diminish?

8. Do they ramify, and in what direction? Do the branches re-unite?

9. In what order are the minerals arranged, of which the vein is composed?

10. Are there any fragments of other rocks, any pebbles, any organic remains among them?

11. When a vein comes in contact with a different species of rock from that in which it was first observed, is the vein abruptly cut off, raised, depressed, turned aside, or are its materials altered?

12. If a vein is cut off, or shifted by the interposition of a stratum or mass of rock, does it reappear or recover its direction on the other side of the interposed body?

13. Is it shifted or cut off without any apparent cause?

14. Are neighbouring veins composed of the same materials?

15. Have veins, consisting of similar materials, the same direction?

16. What proportion do the several veins bear to the rock in which they are found?

17. Do they run parallel to each other?

18. Do they tend to a common centre?

19. Do they cross each other? and what phænomena occur under these circumstances?

20. What is the nature of their floor, sides, and roof?

21. Do the veins seem to have produced any change on the adjacent part of the containing rock, as indurating it, disturbing the regularity of its stratification, &c.?

21. Can they be traced to beds composed of the same materials as themselves?

§ XI. *Concerning Organic Remains.*

1. To what class, and species, do they belong?

2. Do they conform to the direction of the strata in which they occur?

3. Do particular shells, &c. affect particular strata?

4. What change have they undergone? Are the vegetables compressed, carbonized, bituminized, silicified, or penetrated with pyrites in whole or in part? Do the shells retain their enamel? the bones their phosphoric acid, &c.?

5. Do the shells or other organic remains appear perforated or worm-eaten?

6. What

6. What is the nature of the rock or bed in which they are found?

7. Are the bones disposed in entire skeletons? are those of different animals mingled together?

8. Are the shells worn, broken, crushed, or thrown out of their natural position? Are the different species confusedly intermixed?

9. Does this mixture extend not merely to species and tribes, but even to classes? *i. e.* are the remains of fish and sea shells accompanied by those of land animals and vegetables?

10. Are any analogous living species now found, or known to have been formerly found, in their vicinity or elsewhere?

11. Among the various organic remains, can any traces be observed of the existence of man?

XCIX. *Memoir on the Poison of the Viper. By Professor* Mangili*.

The ancients believed that the poison of the viper when introduced directly into the alimentary canal, was not productive of any injurious effect to the animal œconomy. They grounded this belief on the fact, that a person may with impunity suck a wound inflicted by this animal; and this was even prescribed as one of their remedies. The same opinion was adopted by Redi.

More lately Fontana maintained that though a small dose of poison might be imbibed without danger, a greater dose would produce the most serious consequences—even death. He killed eight vipers, and extracting all the venom out of them, he introduced it into the stomach of a pigeon which had not eaten for eight hours before. In less than a minute the animal appeared sickened; at the end of two other minutes it began to stagger, fell on its side, and died in six minutes in strong convulsions.

This experiment was contrary to one made by Redi, who, having diluted in a glass of water some poison extracted from the heads of four vipers, and having given a part of it to a kid, and the rest to a water-dog, no sort of harm resulted from it.

At last Jacob Sozzi drank with equal impunity the poison of a viper diluted in half a glass of wine; at another time he drank in the same manner the poison of three vipers.

With the desire of clearing up this matter, the author of this memoir made at first experiments on four young blackbirds. The first swallowed the venomous fluid of three vipers;—the second that of four;—the third that of five;—and the fourth that

* From the *Giornale de Fisica, Chimica*, &c. vol. ix.

of

of six. For some time they appeared plunged in a state of stupor and inertness; but scarcely had one hour elapsed before they showed themselves as vivacious and hearty as before.

So convincing were these experiments to one of the assistants, that he swallowed all the poison which could be extracted from four other large vipers, and was not in the least affected by it.

The following year the experiment was repeated on a crow after a fast of twelve hours, and it swallowed without injury the venom of six vipers.

In the month of October 1814 (continues the author of the memoir) I made seven large vipers discharge all their venom into one dish, and immediately dipped into it four small pieces of bread, which were swallowed by a pigeon. At first he appeared languid, but in a short time became as well as before. Some days after I inserted into one of his feet, as also into those of another pigeon, a small particle of very dry venom, which had been preserved for four months in a glass phial well corked up. Both of them soon gave manifest signs of being poisoned, and died at about the end of two hours.

Another pigeon swallowed all the venom which ten of the largest vipers could furnish, without presenting the least symptom of being poisoned.

Fontana alleged that the dry poison does not preserve its poisonous properties beyond nine months. This assertion was founded on experiments in which there was a possibility that the poison introduced into the wound might not have been retained, but expelled again with the flow of blood; and it is besides completely refuted by the following experiments of the author of this memoir. On poison preserved carefully for eighteen months, twenty-two months, and even twenty-six months, being introduced into the feet of several pigeons, they all died within half an hour or an hour. To secure the retention of the poison, the author immediately on inserting it stopped up the wound with a little lint. The result of these experiments completely demonstrates the error of Fontana's assertion, and proves that the poison of the viper preserved with proper care may retain for many years its destructive properties.

C. *The Phænomena called by the Name of Gravitation proved to be proximate Effects of the Orbicular and Rotary Motions of the Earth. By Sir* Richard Phillips.

It is the proper object and end of philosophy to investigate the mechanism of causes, and the means by which they produce natural

natural phænomena. For this purpose observers register facts, and philosophers infer the causes from the phænomena by a logical process of induction.

The design of the present essay is to determine the causes of all those phænomena, on which philosophical observers have hitherto conferred the name of Gravitation or Attraction, and which is vulgarly designated by the name of Weight. Owing to what cause or causes does a body fall to the earth?—Why does a projectile return to the earth?—These are the questions which it is here proposed to answer.

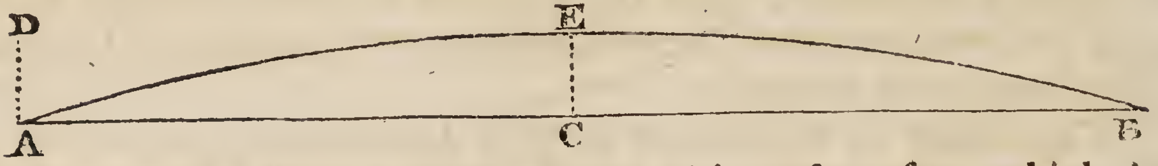
The Newtonians, and all the modern schools of philosophy, have been unable to solve these problems; or, finding themselves unable, they have been unwilling to discuss them, or even tolerate their discussion: while the Theologians have been desirous of referring this power to the proximate agency of the Deity. It is, however, the duty of genuine philosophers to persevere in spite of difficulties and obscurities; and of wise theologians to exalt their notions of the Deity by contemplating the sublime and simple mechanism of secondary causes.

In the present case, the phænomena consist in the apparent influence of one body upon another, though they are not in contact, and though no visible, mechanical agency appears to exist between them; and in their approach to each other by certain laws of accelerated motion, as a result of apparently continued and reiterated forces.

What, however, are the circumstances in which the bodies so acting upon each other are placed, as in the case of a stone projected from, or falling to, the earth? The Earth is a globe of heterogeneous materials, moving round the sun in every year, at such distance, that its mean rate of motion, in round numbers, is 100,000 feet in a second of time; and the stone moves with the earth in the same orbit, partaking conjointly with it of the same mean motion of 100,000 feet in every second. Nor will any one doubt that, at the time the earth and atmosphere are performing this orbicular motion, they are also performing a rotary motion in every twenty-four hours, which rotary motion carries bodies on the earth's surface through a space, at the equator, of 1250 feet in a second, or one-eightieth of the orbicular motion.

The whole earth, then, with all bodies upon the earth, and the atmosphere, are subject to these combined motions and forces; and, in this passive state, the questions are: by what law or laws the heterogeneous particulars are kept together? and how, if the positions are disturbed, those positions are restored?

Suppose



Suppose A to be a place on the earth's surface, from which, by muscular or explosive force, a stone is projected towards D, at sixteen feet and an inch above A. Suppose that a second of time elapses while the stone is ascending from A to D, then it is evident that the point A will in that second be carried forward, by the orbicular motion of the earth, 100,000 feet, or to C; that is to say, the point A will move 100,000 feet while the ball is ascending sixteen feet and an inch: consequently the ball will, in truth, not ascend in space from A to D, but will be carried in an oblique line from A to E, moving upwards as it proceeds. The two forces—that which carries it from A to C, and that which carries it towards D,—are as 100,000 to $16\frac{1}{12}$, or as 6000 to 1 nearly.

The stone having arrived at E, it is then known, by the phænomenon, to fall to the earth in a second of time; yet it does not fall through EC, but, while falling, is carried, by the orbicular motion, through 100,000 feet to B, in the diagonal EB.

Such a diagonal course as AE is therefore, in point of fact, generated by every projectile, while it is rising to any given height in the atmosphere; but, as the spectator is carried with the projectile, he merely measures its novel and peculiar motions, and is incapable of observing the orbicular motion, of which he partakes in common with the projectile. The projectile is not, however, the less subject to the force of the orbicular motion, because it is not perceived or felt by a spectator at rest; but, as a body put into *motion* by *novel forces*, acting in *opposing* directions, its novel state of opposing motion is liable to be affected by all opposing forces simultaneously existing in nature; and, when its novel force is exhausted or destroyed at E, it becomes the patient of the great natural forces, which, in moving the earth and atmosphere from A to B, move it likewise, as part of the system.

The projectile having however ascended to E, a difficulty arises in regard to the origin of the force which deflects it from the summit at E towards B. Why does it not move for ever in the parallel DE? What is the original force that turns it aside? Is that force required to be equal to the weight of the body; or what proportion of that force, and how is it generated?

It appears by the fact that the whole force which was necessary to cause it to fall through the $16\frac{1}{12}$ feet, is but a 6000dth part of the orbicular momentum; consequently the deflection of 16 feet forms but an angle of 20 seconds at A and at B. The nascent

nascent deflection, however, at E would be but an infinitesimal of the said 20 seconds; consequently, any indefinitely small deflective force, arising from new combinations of the forces, might be sufficient to return a body to the earth.

What then is the actual deflective force which turns a projectile downward, and prevents it from moving for ever in the place in the atmosphere in which it has been left by the extinction of that projectile force, which carried it from the surface into so novel a situation in the atmosphere?

Let us examine all the circumstances in which the body has been placed:—

1. *It was moving with the earth in its orbit with all other bodies upon the earth, and therefore possessed a momentum in that direction, which, with regard to other bodies, was as their quantities of matter.*

2. *It was deflected aside by some novel muscular or explosive force, and thrown towards* D.

3. *But, while it was moving towards* D, *or* 16 *feet and an inch, it was carried* 6000 *times as far, or* 100,000 *feet at right angles; and therefore performed a diagonal.*

4. *At* C *it loses its force in the direction at* D, *and is deflected towards the earth, at* B.

Let us now examine what other circumstances have attended its ascent:—

1. *Its passage has been made in a resisting medium, which tended, as is known, constantly to destroy the force with which it was projected from* A *towards* D.

2. *During its ascent the point* A, *and every point of the diagonal* AE, *were also performing a rotary circular motion round the centre of the earth.*

3. *The point* A *would therefore be deflected downward, during its passage towards* C, *from* A *below* C; *so that* D *would also be deflected below* E, *and the entire line* AE *would be deflected, or turned downward, below* AC.

4. *The rotary motion would therefore have the effect of deflecting the body below the diagonal* AE, *at every increment of its ascent.*

5. *The constant resistance of the atmosphere would, in like manner, deflect it.*

6. *The body would, therefore, be subject, during its ascent, to the action of four forces:*

a, the *orbicular force.*
b, the *projectile force.*
c, the *rotary force.*
d, the *resisting force.*

7. *At the apex, the projectile and resisting forces having destroyed*

stroyed each other, the body is then surrendered to the joint action of the orbicular force and the rotary force.

8. *And we have seen that it is the effect of the rotary force to deflect it from the right line of the orbicular force towards the earth.*

Consequently it is the rotation of the earth and atmosphere, acting simultaneously with the annual motion, which produces the deflection of bodies from the right line of their orbicular motion; but the circular rotation has another important effect on the masses of various density which compose the earth, and which, as subject to a common force, would have different orbicular velocities, but for the effect of their common rotary motion in circles of different radii*.

To the rotary motion of the earth is, therefore, to be referred that uniformity in the velocity of bodies of various density, which enables the whole to keep an equal and uniform pace in the orbit of the earth. It is this rotary motion which reduces to order, what otherwise would be chaos. Hence it is that all fluids are impelled into a level surface; hence too, doubtless, it has been, that masses of the same density have formed themselves into strata while in a state of solution; hence arise all the phænomena which result from any disturbance in the order of density; and hence it is that, when a heavy body is thrown into lighter fluids—as air or water—the general law is proved by phænomena exactly proportioned to the relations of density.

We know, from the diurnal phænomena, that the earth and atmosphere have such a common rotary motion; and we know, from their common orbicular force, that the whole have unequal momenta. It is, however, a necessary mechanical effect of a common rotary motion, producing, of course, equal momenta in masses of various density that they range themselves, or seek to range themselves, in concentric circles, or radii of rotation, inversely, as their respective densities. The lighter bodies will be projected from the centre towards the circumference, and the

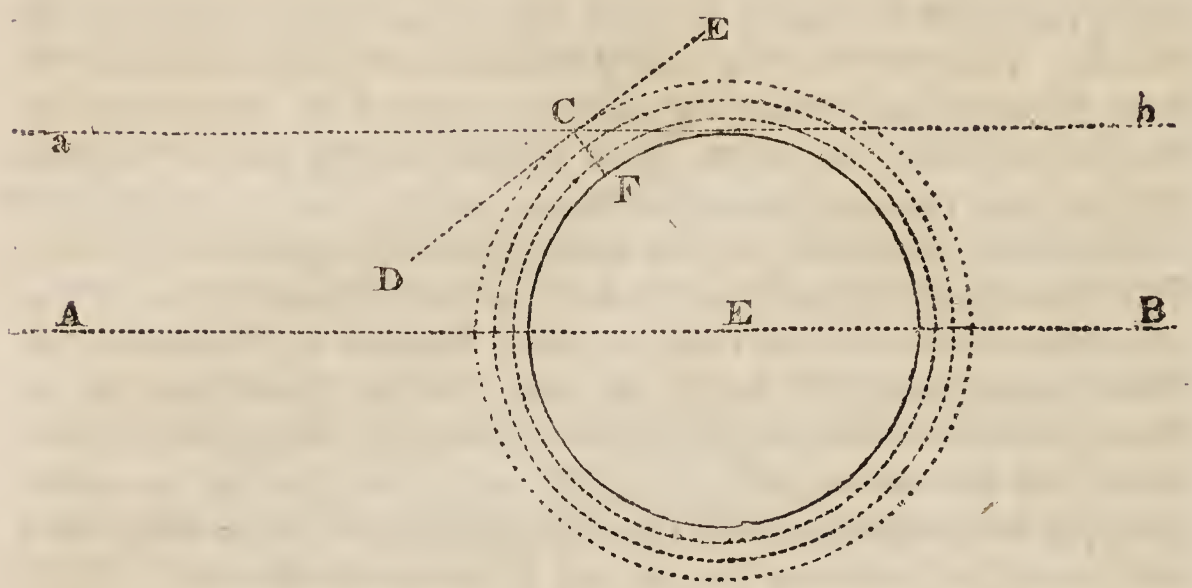
* It seems extraordinary that, although the two-fold motion of the earth has so long been known and admitted, no mechanical effects should have been considered as arising among its parts from the operation of those motions; and that one motion should have been considered as having no other end than the alternation of seasons, and the other none but the changes of day and night. It is true that these are some of the ends resulting from the two-fold motion; but general causes have many effects, and these are rather consequential than proximate. The proximate effect of motion is force; the proximate effect of force is the communication of motion, force, and momentum: and in this instance the motions of the earth produce in the parts motion, force, and momenta: which, diverted, deflected, or turned aside, by any foreign muscular, mechanical, or explosive force, produce the phænomena of resistance and weight.

heavy

heavy ones will be forced towards the centre by their mutual collisions. Such will be the law, governing all the masses that are free, to move one among another, as the fluid parts and the moving or moveable parts. It is a law growing out of the conditions, and the conditions are those which exist in Nature. If a projectile, therefore, have a specific density equal to the air or fluid into which it has risen, it will be carried round the earth in the concentric circle of that stratum, because the momenta are there equal; but, if it be lighter than the air, it will then be reflected by the denser strata, till its own momentum, and the momenta of the surrounding air, be equal; while, on the other hand, if it be heavier than the circumambient air or fluid, then the air or fluid will rise over it and deflect it to the earth, with a force which will be nearly in the ratio of their densities.

Galileo was the first geometrician who analysed the phænomena of falling bodies, and determined the law of their motions. He was followed in this theory by Newton, and his doctrines are justly respected to this day. That great man considered, however, that bodies thrown perpendicularly upward, merely describe in rising and falling the same straight line. He was one of the most powerful advocates of the two-fold motions of the earth; yet he never considered bodies acted upon by a temporary and relative projectile force, as still subject to the absolute two-fold motions of the earth and atmosphere. Hence he considered the force which returned the body to the earth, as equal in small distances to the weight of the body, and as acting in right lines from the centre of the earth. In this notion he was borne out by the belief in all kinds of sympathies and emanations which characterized his age, as well as that of Newton; and to these superstitions may, doubtless, be referred the doctrine of an emanating gravity. It appears, however, that, as a body subject to the novel force of an upright projectile, does not, in truth, describe a perpendicular line, but two sides of an exceedingly obtuse triangle; no force is requisite to deflect it to the earth, but the exceedingly small one which creates the nascent deflection. Hence, as the angle of deflection required to carry a body through the first inch, is not the 2000dth part of a degree; the deflective force need not be more than the 120,000dth part of the permanent momentum of the body created by the orbicular force: and consequently no difficulty arises in referring the small deflective force to the combinations of the great motions of our planet.

 Suppose



Suppose E to be the earth ; AB a portion of its orbit ; F a place on the earth's surface ; C a stratum of the atmosphere which a projectile has reached ; *a b* a parallel of the orbit, and DE a tangent of the atmosphere ; then it is evident that, whether the earth is turning from D towards E, or from E towards D, a deflection, from the orbicular motion to the rotary, would take place, measured by such an angle as D C*a*, or *b*CE, in either case creating a direction of force opposed to that in the orbit, and producing a resistance in the next superior atmosphere, and creating a deflection downward fully equal to the known phænomena, which requires for the first deflection but an infinitely small portion of an angle of 20 seconds.

It is also evident that the common rotary motion of the earth and atmosphere would confer a greater velocity on the circle C than the circle F, and so in every circle from C to F, the spaces described being as the squares of the radii, or as the squares of the distances from E. To confer, however, an equal momentum on the mass, it is necessary that the densities from C to E should be inversely as the velocity, or inversely as the spaces, or inversely as the squares of the radii.

Moreover, as by the phænomena, the momenta in every concentric stratum or circle of rotation are known to be equal, so any mass of equal density would in different circles, or at different distances, be carried or impelled in the vortex by the circumambient media, or incumbent bodies, through spaces, which would be to each other, *inversely as the squares of the distance from the centre, and directly as the resistance of the medium in which they move.*

Consequently, the collision of the bodies or masses of different densities, in the terrestrial system, will force or urge the heavy bodies towards the centre, and of course, also, the light ones towards the circumference—circumstances which we know accord with

with the phænomena of all fluid bodies, and of all fixed bodies when deprived of competent support, and of all bodies moved out of their station by muscular or explosive force, and left without support. Q. E. D.

The following illustrations and observations may tend to make these principles perfectly clear, and to remove all doubts.

1. The projectile at its apex, when deprived of the projectile force, is still immersed within the atmosphere, and is carried, or urged, by the atmosphere, in the direction of the circular rotation of the atmosphere.

2. That circular rotation then produces, or solicits to produce, (with a force proportioned to the relative densities of the atmosphere and projectile,) a deflection of the said projectile from the right line of the orbicular motion, into the direction of the circular motion at the place of contact.

3. If the density of the projectile were equal to the density of the atmosphere, then the projectile would float in the atmosphere, and be carried round the earth in the circular vortex of the earth, like the atmosphere itself.

4. If it were denser than the atmosphere, then the rotary momentum of the lighter atmosphere being less than the rotary momentum of the projectile, resistance would be generated equal to the difference of their rotary momenta; and a deflection of the denser body by the lighter atmosphere, into a smaller circle of rotation, would take place till it reached a competent basis on the earth.

5. If the projectile were lighter than the atmosphere, then the momentum of the circumjacent atmosphere would be greater than that of the projectile, and the projectile would, in consequence, be forced upward into a larger rotary circuit, till its rotary momentum equalled that of the circumjacent atmosphere.

6. In media of uniform density, as in water, the projectile, if heavier, sinks to the bottom; if lighter, floats on the top, equalizing its momentum by presenting a portion of its mass within the air; or, if of the same weight, floats indifferently within the fluid.

7. As the deflective force is continually operating during the fall, and as every uniformly continued force generates a constantly accelerated motion, so as bodies fall by the action of a continued deflective force, they are necessarily accelerated during their fall, till they rest on the fixed parts of the earth. But, as is found by experiment, the increased resistance of the air, arising from the increased motion, will occasion a balance of the two forces, and an equable motion during part of the fall.

8. Combining the time of falling, the spaces as determined by Galileo, and by alleged experiments, with the actual bases generated by the orbicular motion, the following will be the ele-

 ments

ments of bodies falling in times between four seconds and the tenth of a second:

Seconds.	Feet.	Base.
4	256	400,000
3	144	300,000
2	64	200,000
1	16	100,000
·5	4	50,000
·25	1	25,000
·1	$\frac{1}{12}$	10,000

9. As every successive circle, from the centre to the utmost bounds of the atmosphere, in performing the diurnal rotation, contains bodies of such density, that the density multiplied by the motion is equal; so no body of undue density would remain in a circle of rotation which created in it too great or too small a relative momentum, provided, as in fluids, the parts were free to move one among another; and, if they were not free to move, as in fixed organizations, then they would exhibit the phænomena of pressure, or would appear, in regard to other bodies, to solicit to ascend or descend, as the case might be. Probably to this conflict of light and heavy bodies, to the generation of light bodies in undue positions, and to the varied action of fixed and fluid matter, may be referred most of the phænomena of terrestrial organizations; and, as the whole grows out of the two-fold motion of the earth, so we thus refer the origin of all things and phænomena to the agency of motion.

It follows, therefore, that, the velocity of all the parts and dependencies of the earth being alike, the momentum of every part is directly as its quantity of matter; consequently, MOMENTUM IS WEIGHT; and the velocity of 100,000 feet per second in the direction of the tangents of the orbit being so much greater than the ordinary, or even extraordinary forces applied to move bodies in any novel direction, the phænomenon called by the name of Gravitation *is a necessary result of a preponderating, coincident motion in another direction*, all contrary or foreign motions being ultimately destroyed by the resisting medium of the atmosphere and the deflective circular motion.

The weight of bodies, then, acts on the same principle as their momentum in the orbit of the earth, because every force which exhibits the phænomena of weight affects the paramount orbicular force; and, *this active principle is, therefore, merely a phænomenon resulting from the orbicular momentum, regulated by the rotary motion;* which rotary motion causes all bodies to perform circuits inversely as their quantities of matter, and which circuits are proportioned to the distance from the centre.

The phænomena of weight and gravitation, and of falling bodies,

bodies, may therefore be considered as a result of the composition of forces produced by the annual and diurnal motions of the earth and its adjuncts. Of course, all the motions and laws of projectiles are subordinate to these, and are included within them.

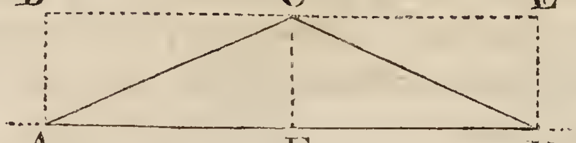
On a small scale, the principle may be illustrated by experiments of analogous phænomena, in which it will appear that any lateral motion of bodies produces the same results as the earth itself; and that, as a consequence of every such lateral motion, the projectiles from such bodies respect, in falling, the bodies whence they are projected, though no suspicion or allegation of attraction can, in such cases, be adduced.

Thus, as it is well known, a ball projected from a ship in motion falls at the place whence it rose, though, during its ascent and descent, the ship proceeded many yards. The ball, though detached from the ship, respected the ship, during its flight and fall, exactly as though it had been attracted by the ship. It cannot, however, be contended that such phænomena are results of the ship's attraction! The effect arises merely from the ship's lateral motion having been acquired by the ball previously to its projection; the case being exactly the same as that of a ball projected above the earth, to which it falls again, on the very principle on which it fell on the ship, not by the attraction either of the ship or the earth, but because the earth's lateral motion, like the lateral motion of the ship, had been acquired by the ball.

So, also, when a man, during feats of horsemanship, throws up oranges in his course, he readily catches them again, though during their flight he advanced many yards, because his lateral motion had previously been given to the oranges; and in consequence, in falling again, they respected him just as though, according to pre-existing notions about the earth, he had attracted them.

In like manner, if a ball be dropped from the top of the mast of a ship, which at the time was moving at any given rate, the ball will fall perpendicularly at the foot of the mast, exactly as though the ship had been standing still; and will thus appear to be influenced by the ship, and not by the earth, exactly as a ball let to fall from a tower falls to the earth, and appears to respect the earth. In both cases, the phænomena are regular and necessary consequences of the composition of motion; and there is no unseen or occult power, called attraction, concerned in one case more than in the other.

ILLUSTRATION.



If a ball be projected from A perpendicularly towards D, and A be supposed to be on the deck of a ship, which is moving at any given rate, from the part A towards the part B; then the ball, instead of ascending to the point of space at D, will, without material error, move in the diagonal AC, by the compound forces which, in the same time, would have carried it to D or F; and in descending it will fall to the deck at B, to which the point A, in the interim, has advanced. It has in truth performed, instead of an apparent perpendicular, the two sides of the triangle, or the curve ACB; and instead of falling at the point of space A, whence it was projected, it has advanced with the ship, and fallen at the point of space B, to which A has advanced; and in falling has respected the ship, and not the point of space, or the earth at A. It was not however attracted by the ship, but merely impelled in the diagonal by the motion of the ship, in which it previously participated.

These cases fully illustrate the principle for which I contend; but, of course, the relations and powers of the ship, and man moving on the moving earth, are included within the more extensive relations and powers operating in Nature, in the prodigious forces generated by the annual and diurnal motions of our planet. The case of a body thrown upward from a ship in motion, merely shows that, in falling, it obeys a law growing out of the motion of the ship; in like manner as bodies projected from the earth, or falling towards it, obey a similar law of motion growing out of similar motions of the earth. The law in both cases is a mere result of the composition of motion, and not an effect of any occult or mysterious power.

These examples, and others that might be adduced, serve, however, to prove the exact analogy of the powers, the phænomena, and the results. No experiments in physical philosophy seem to afford more conclusive analogical proofs of doctrines relative to any natural operations, which are too vast to be viewed by man on their great scale.

The recognition of the principles of this paper may be expected to lead to more precision in the doctrines of projectiles. Many incomprehensible irregularities in Dr. Hutton's and Mr. Robins's experiments on gunnery may be found to arise from the contrariety, opposition, or neutrality, of the direction of the rotary

tary motion, in relation to that of the orbicular motion. It will likewise be found that periodical fluctuations in the weight, and deflections of the atmosphere and other fluids, may be traced to periodical variations in the relative directions of the mechanical forces, as in the tides, monsoons, trade-winds, &c.

To extend these principles to the solution of the phænomena of Nature, and to deduce from them all the results of which they are susceptible, would be, in regard at least to nomenclature, to remodel the "*Principia*" of Newton, and to arrange a new system of physics.

Nevertheless, I feel it proper to state, that these demonstrations of the true and necessary causes of the phænomena hitherto ascribed to an unknown power, called by the name of Gravitation, merely fill some important connecting terms in the series of mechanical causes ascertained by modern philosophy; while they disturb none of the known relations of bodies, as determined by experiment and observation, or by the geometrical and analytical investigations of Galileo, Kepler, Descartes, Newton, Euler, La Grange, Herschel, or Laplace.

A Summary of these doctrines may, perhaps, without material error, or omission, be expressed in the following paragraphs:

1. That bodies moved in the annual orbit of any planet, acquire a momentum in the direction of the increments of that orbit superior to the influence of any other permanent force which is communicated to them.

2. That all variations in the direction of this orbicular motion are effected by deflections of that paramount motion, either by the rotary diurnal motion, or by some muscular, mechanical, or chemical force.

3. That the resistance which bodies exhibit in being lifted, or thrown, or in any way turned into any new direction, is the measure of their weight, which is as their quantity of matter, because it arises from a velocity common to the terrestrial system in the direction of the orbit.

4. That the phænomena of falling bodies are produced by the deflection of the circular rotary motion from the comparatively straight line of the orbicular motion.

5. That every body which has had any new direction of force given to it, is nevertheless subject to the permanent influence of the pre-existing orbicular and rotary forces in the lines of their direction, and the resulting line of motion is the effect of all the operative forces.

6. That the phænomenon of the ascent and descent of bodies is also influenced by the resistance of the medium through which its novel direction of motion has forced it to proceed.

7. The force with which the deflection by the rotary motion is

is produced, is as the density of the body deflected to the density of the medium in which it moves, and in the inverse ratio of the squares of the distance from the centre.

8. That it is the necessary tendency of the rotary motion to give an equal momentum to the heterogeneous masses composing a planet and its atmosphere, while the whole are moving with an equal velocity in the orbit.

9. That all phænomena of motion visible to beings who partake of the common motions of a planetary system, are either the relative motions of distant orbs, or the disturbance of the great common motions by new directions of motion produced by some muscular, mechanical, explosive, or chemical force.

10. That these temporary and novel directions of force and motion are speedily extinguished by the great permanent forces moving in other directions, all traceable to the phænomena, and producing compositions of motion which result from the known laws of dynamics.

11. That of course similar motions produce similar phænomena in all planetary bodies.

12. That, therefore, the phænomenon hitherto ascribed to an occult power called gravitation, is a simple result of known motions.

The application of these principles to the phænomena of a system of bodies moving within the gaseous medium of universal space, will be the object of some future paper.

Holloway, June 10, 1817.

CI. *On Mr.* Liston's, *or the* Euharmonic *Scale of Musical Intervals, extended according to his tuning Process, from* 59 *to* 612 *Notes in the Octave; showing thus, a Division of the Octave into* 612 *equal Parts, or as nearly so, throughout, as Experiments in Harmonics, or the most refined musical Performances, seem to require.*

By Mr. John Farey, *Sen.*

To Mr. Tilloch.

Sir, — It has been truly remarked, that, "By stopping in the investigation of any subject, short of its thorough or *general* investigation, we are liable to fall into the error, of mistaking particular cases, for general facts, and of too soon drawing conclusions, which on more matured consideration, require to be recalled or amended." Thus it has in a small degree happened, with regard to one of the positions respecting the Euharmonic or Listonian Scale of Musical Intervals, in my paper on the Nomenclature of these Intervals, which you did me the favour to insert in

in p. 364 of your last number: Wherein, after assigning the symbol ✠ to the Interval $83\Sigma+2f+7m$, I have mentioned, that the same is *always* the value of a double sharp (**), or a double flat (bb);—which last, although true in all the instances of a Sharp added to a Sharp, or a Flat to a Flat, as C to C** and Cb to Cb^3 (or three times flattened), &c. yet it fails in a very few other cases of Mr. Liston's Scale: because, from an extension of his Euharmonic Scale of Intervals, so as to produce a Note corresponding to every one of the 612 *Artificial Commas*, into which the Σ's of my Notation divides the Octave* (which extension I have effected, *since* the paper referred to was written) I have lately found, that the following Intervals, viz. between C* and Cb, between C'* and C'b, between C''* and C''b, between C'^3* (or three times acuted) and C'^3b, and between C'^4* and C'^4b; also between $C^{\backprime}$* and $C^{\backprime}$b, between $C^{\backprime\backprime}$* and $C^{\backprime\backprime}$b, between $C^{\backprime 3}$* (or three times graved) and $C^{\backprime 3}$b, between $C^{\backprime 4}$* and $C^{\backprime 4}$b, and between $C^{\backprime 5}$* and $C^{\backprime 5}$b, are each of the value 2I, or $2\mathfrak{S}=94\Sigma+2f+8m$, respectively; but in all other instances, of this very extended new Scale, the values of double Sharps, or double Flats, or of the sums of single Flats and single Sharps, of the same Notes (as from G* to Gb, D* to Db, &c.) is the *Tone* minimum, or $83\Sigma+2f+7m$, as has been stated, in the page referred to, but without these qualifications, of its *general* application, here given.

The happy discovery, by Mr. Liston, of a Tuning process, which, by help of *perfect* major *Thirds*, *Fifths*, and *Octaves* only, is found capable of giving any required extension to the Scale, still preserving throughout the same, a true harmonic relation to the note first assumed, or C (as mentioned in p. 421 of your 39th volume, and which the late Mr. Maxwell failed in effecting) has led me to the arrangement, of the extended scale here spoken of, in a Table, composed of numerous small squares, which has the effect of exhibiting its many curious properties, in a very striking manner. The bulk of this table is too considerably extended, by the many squares in the opposite corners of the sheet, which are unfilled, for it to seem probable, that you could give this Table a place in your Magazine; otherwise, for the sake of the insight which it

* It appears, however, and the Student must always bear it in mind, that these divisions, although extremely near to it, are not exactly *equal* amongst themselves, a thing impossible to see effected, with regard to the aliquot division of any prime musical Interval whatever; but of the 612 intervals or differences which occur herein, between 0 0 0, and $612\Sigma+12f+53m$, 441 of these, have exactly the value of Σ 0 0; 118 of these differences are of the value $\Sigma+f$, and have the ratio, $3^{29}\div2^{39}\times5^3$, or $1{\cdot}14966096\times\Sigma$: and the remaining 53 of them have the value $\Sigma-2f+m$, and ratio $3^2\times5^{15}\div2^{38}$, or $\cdot70854048\times\Sigma$;—or, near enough for every practicable purpose, my 612 artificial commas in the octave, always have differences between them, either $\frac{71}{100}$ths, $\frac{100}{100}$ths, or $\frac{115}{100}$ths of a *Schisma*.

affords,

affords, into the mysteries of this curious subject, I should have been very glad to have seen this done. I will, therefore, now endeavour, to give such directions, as may easily enable any of your curious Readers, to construct such a Table of Intervals for themselves.

The Key Note C, with its artificial Comma under it, or $\underset{0}{\text{C}}$, is placed in a central square of my Table: the squares on its right hand, in the same horizontal range, are occupied by the following succession of Vths, viz. $\overset{\text{G}}{358}$, $\overset{\text{D}}{104}$, $\overset{\text{A}'}{462}$, $\overset{\text{E}'}{208}$, $\overset{\text{B}'}{566}$, $\overset{\text{F}'\ast}{312}$, $\overset{\text{C}'\ast}{58}$, $\overset{\text{G}''\ast}{416}$, $\overset{\text{D}''\ast}{162}$, $\overset{\text{A}''\ast}{520}$, $\overset{\text{E}''\ast}{266}$, $\overset{\text{B}'^{3}\ast}{12}$, $\overset{\text{F}'^{3}\ast\ast}{370}$, $\overset{\text{C}'^{3}\ast\ast}{116}$, $\overset{\text{G}'^{3}\ast\ast}{474}$, $\overset{\text{D}'^{3}\ast\ast}{220}$, and $\overset{\text{A}'^{4}\ast\ast}{578}$: the same horizontal column containing, in reversed order, or towards the left hand, the Vths, $\overset{\text{F}}{254}$, $\overset{\text{Bb}}{508}$, $\overset{\text{E}^{\backprime}\text{b}}{150}$, $\overset{\text{A}^{\backprime}\text{b}}{404}$, $\overset{\text{D}^{\backprime}\text{b}}{46}$, $\overset{\text{G}^{\backprime}\text{b}}{300}$, $\overset{\text{C}^{\backprime}\text{b}}{554}$, $\overset{\text{F}^{\backprime\backprime}\text{b}}{196}$, $\overset{\text{B}^{\backprime\backprime}\text{bb}}{450}$, $\overset{\text{E}^{\backprime\backprime}\text{bb}}{92}$, $\overset{\text{A}^{\backprime\backprime}\text{bb}}{346}$, $\overset{\text{D}^{\backprime 3}\text{bb}}{600}$, $\overset{\text{G}^{\backprime 3}\text{bb}}{242}$, $\overset{\text{C}^{\backprime 3}\text{bb}}{496}$, $\overset{\text{F}^{\backprime 3}\text{bb}}{138}$, $\overset{\text{B}^{\backprime 3}\text{b}^{3}}{392}$, and $\overset{\text{E}^{\backprime 4}\text{b}^{3}}{34}$.

The vertical column of squares passing through C, is in like manner occupied, upwards, by the following succession of IIIrds, viz. $\overset{\text{E}}{197}$, $\overset{\text{G}\ast}{394}$, $\overset{\text{B}\ast}{591}$, $\overset{\text{D}^{\backprime}\ast\ast}{176}$, $\overset{\text{F}^{\backprime}\ast^{3}}{373}$, $\overset{\text{A}^{\backprime}\ast^{3}}{570}$, $\overset{\text{C}^{\backprime}\ast^{4}}{155}$, $\overset{\text{E}^{\backprime}\ast^{4}}{352}$, and $\overset{\text{G}^{\backprime}\ast^{5}}{549}$: and the same column also contains, downwards, the IIIrds, $\overset{\text{Ab}}{415}$, $\overset{\text{Fb}}{218}$, $\overset{\text{Dbb}}{21}$, $\overset{\text{B}'\text{b}^{3}}{436}$, $\overset{\text{G}'\text{b}^{3}}{239}$, $\overset{\text{E}'\text{b}^{4}}{42}$, $\overset{\text{C}'\text{b}^{4}}{457}$, $\overset{\text{A}'\text{b}^{5}}{260}$, and $\overset{\text{F}'\text{b}^{5}}{63}$.

Besides which, two other short ranges of IIIrds are added, one at bottom of the Table, not under C, but beginning under D, (with $\overset{\text{E}''\text{b}^{6}}{582}$), and extending towards the right hand; and the other at top, beginning over Bb (with $\overset{\text{A}^{\backprime\backprime}\ast^{5}}{30}$) and extending towards the left hand.

Although the C (or 0 IIIrds) range of Vths, proceeds no further than 17 towards the left hand, yet the $\text{C}^{\backprime}\ast^{4}$ (or +7 IIIrds) range, proceeds in like manner 7 Vths further towards the left hand, and others of the ranges above the C range, do the same, in lesser degrees, occasioning the Table to swell out towards the top left-hand corner, as already hinted: And in like manner the $\text{C}'\text{b}^{4}$ (or −7 IIIrds) range, extends out to the number of 24 Vths, towards the bottom right-hand corner of the Table, and other ranges near, in lesser degrees.

Thus

Thus it happens, that the whole Table is composed, of 21 horizontal ranges, of different lengths, of *consecutive* perfect Vths *, differently placed over each other, so as to form, at the same time, 49 vertical ranges of *consecutive* perfect IIIds†, of different lengths, and positions.

The artificial Commas, at the left-hand ends of the 21 ranges of Vths, beginning at the top, are 180, 179, 178, 131, 292, 95, 510, 417, 324, 485, 34, 553, 460, 263, 66, 227, 388, 295, 306, 63, and 582, respectively; and those at the right-hand ends, 30, 549, 306 ‡, 317, 224, 385, 546, 349, 152, 59, 578, 127, 288, 195, 102, 517, 320, 481, 434, 433, and 432, respectively.

And the number of Vths in each of the 21 ranges above mentioned, beginning at the top of the Table, are 4, 14, 27, 34, 35, 36, 37, 35, 33, 34, 35, 34, 33, 35, 37, 36, 35, 34, 27, 14 and 4, respectively, which together, amount to 613, the number of artificial Commas, and of Notes here, in the Octave, including both Bb'^{5} and $D*^{5}$, in the middle of the Octave, which reckon as one Note, by the artificial Commas, although differing by 2f+m.

The number of Notes in this Table, without either *açute* or *grave* marks (′ or ‵), is 75. Of those bearing one *acute* mark (′) it is 74, of those with two acutes (″) 70, with three acutes (‴ or $'^{3}$) 51, with 4 acutes ($'^{4}$) 40, with 5 acutes ($'^{5}$) 26, and with 6 acutes ($'^{6}$) the number of Notes is 8; in all 269 acuted Notes. Of those Notes bearing one *grave* mark (‵), the number is 74, with two graves (‶) it is 70, with three graves ($`^{3}$) 51, with 4 graves ($`^{4}$) 40, with 5 graves ($`^{5}$) 26, and with 6 graves b(‵) the number of Notes is 8: in all 269 graved Notes.

The number of Notes without either *Flat* or *Sharp* is 83; the number bearing one *Sharp* (*) is 77, of 2 Sharps (**) it is 63, of ($*^{3}$) 47, of ($*^{4}$) 37, and of ($*^{5}$) it is 17: in all 241 sharpened Notes. Also, the number of Notes bearing one *Flat* (b) is 83, of (bb) it is 71; of (b^{3}) 54, of (b^{4}) 45, of (b^{5}) 32, and of (b^{6}) it is 4: in all 289 flattened Notes.

The *natural* Notes, *i. e.* without either Sharps or Flats, but

* Whose *differences*, in artificial Commas, are either 358, or (358−612=)−254: and to the terms of which series of Vths, the seven *Literals* F, C, G, D, A, E and B are affixed, in successive series; and these are marked with one or more b's or *'s, in similar progressions; and also with grave or acute marks, forming series of 4, 5, 5, 4, 5, 5, &c, successive marks of the same number and kind, except in one instance in each range of Vths; occasioned by the anomaly in the scale, with regard to C* and Cb, which has been mentioned in page 443, where the successive numbers of graves or acutes is 4, 5, 5, 5, 4, &c.: which appears owing, to the tuning *both ways from* C, a thing which does not occur in any other part of the Scale.

† Whose differences are either 197, or (197−612=)−415.

‡ This Note only, being twice inserted, because being in the middle of the Octave, it was so required, for preserving complements, to octaves, throughout.

bearing

bearing *grave* or *acute* Marks, except 7 of them in the centre, will be found to traverse the Table diagonally, from the top left-hand corner to the bottom right-hand corner*: and the Notes without acute or grave Marks, but bearing Sharps or Flats, (except in the centre, the 7 above-mentioned) will be found to cross diagonally, from the top right-hand corner to the left-hand lower corner, of the large manuscript Table I am describing.

I beg to recommend to Musical Students, the useful exercise, of filling up a Table of these 612 equal Notes, from what is here said: by the study of which, a more perfect insight into the true relation of the Musical Scale, will be had, than by any other means: especially that which is usually resorted to by Teachers and Professors of Music, for explaining the derivation and nature of the general Scale, by means of the major and minor *Tones* and *Semitone*, or T(104), t(93), and S or H(57); which, (in common with almost any other three Intervals) are certainly capable, of correctly expressing musical values, when *negative signs* are used, yet this mode is perplexingly intricate, and indeed, unnatural; because, the progress of Modulation, and the tuning of all Instruments, is conducted by means of the *Concords*, the *Fifths*, *Octaves*, or major *Thirds*, the Notes of the common Chord: and in no instance is it practicable to use T, t or S, for the purpose of Tuning: why then should they be used in deriving or explaining the general Scale, instead of III, V and VIII?, by which it is in fact produced, and whereby (when negative signs are admitted) every Note may as readily be expressed, as by means of S, t and T.

I heartily wish that Mr. Flight of St. Martin's Lane, the Organ Builder, who is himself a good Musician, and a very ingenious Mechanic, were encouraged, by *a Subscription* among Musical

* In the like direction, throughout the Table, *the same Note*, as to the Letter and Flats and Sharps it bears, but always lessening a grave or increasing an acute Mark, in proceeding one step diagonally downwards, will be found to range: and the number of artificial Commas will, each one, increase, eleven from the last one, in this same direction;—and this property of the Scale, when thus tabulated, forms by far the best and easiest mode of examining the accuracy, of a Table, of the extent mentioned; or of increasing this, all around, to any required degree. But the Student is to be apprised, that although every Note thus obtained, beyond the limits of the Table, will, by *the artificial Commas* belonging to it, appear to be *equal* to some one other Note, within the Table: yet, that *very small differences* will still always be found:—it being no more possible, for the same exact value of Interval in Σ, f and m, to return, than for this tuning process, again to reproduce any Note whatever, exactly.

Around the border of my Table there are, if the artificial Commas only were considered, 108 repeated Notes; but these Notes, when strictly calculated in Σ, f and m, are found to differ from those within the Table, respectively, by three different, exceedingly small Intervals; viz. 34 of them differ

Musical Persons (to which I would contribute) to undertake and contrive *an experimental* ORGAN, having a single diapason Stop of 1224, or more, Pipes, tuned to two Octaves or more, of this extended Scale; so contrived, with different sets of Finger-keys and Sliders, that for the purposes of Experiment, any two, three or four or more of the Notes, could be tried together, for deliberately ascertaining the effects on the Ear, of every possible Chord, however compound, and the shades of differences between such.

And with other Finger-keys, of the ordinary kind, twelve in the Octave, to which any of the 612 Notes could be attached at pleasure; by means of which, every species of Temperament of the common Scale, which has yet been proposed, or could be invented, might readily and intelligibly be put to the test of Experiment; although not with absolute mathematical precision, yet with a nearer approach thereto, than has yet perhaps, ever in a single instance been attained, even by the celebrated *Dr. Robert Smith*, in tuning by the method of calculated *Beats*, which he invented. Provided, that the Notes of Tempered Systems, intended to be tried, were calculated to *the nearest artificial Comma*, in every instance (than which nothing can be more easy, by help of the Musical Corollaries which I gave in p. 374 of your xxxvith volume) no greater error than about half a Schisma, or the $\frac{1}{22}$d part of a major Comma, or the $\frac{1}{102}$d part of an Isotonic Semitone, need in any instance be committed, during the trial of any system of Temperament, on such an Instrument, and generally, not half as much:—errors, the smallness of which, the most fastidious Critic or Amateur, might safely lay out of his consideration, in judging of the real and comparative effects, of different systems of Temperament, with the view to the adopting and recommending of that, which is on the whole, best adapted to use on *the Organ*;—if such be not already decided?, in favour of the Mean-Tone, or *perfect Major Third System.*

differ just f, whose ratio is $3^{37} \div 2^{54} \times 5^2$, or $\cdot 14966096 \times \Sigma$; 30 of them differ 2f — m, or $2^{53} \div 3^{10} \times 5^{16}$, or $\cdot 29145952 \times \Sigma$, and the remaining 44 pairs, of these apparently equal Notes, within and without the Table, each of them differ 3f—m, or $3^{27} \div 2 \times 5^{18}$, or $\cdot 44112048 \times \Sigma$:—or, near enough for every practicable or useful purpose, the small differences of the first repeated Notes without the limits of my Table, from those already produced by the tuning process within it, may be stated to be $\frac{15}{100}$th, $\frac{29}{100}$th, and $\frac{44}{100}$th of a *Schisma*, respectively; that is, in every instance, such differences, are *less than half*, what thus strikingly appears to be, *the* UNIT *of the Euharmonic Scale* (as observed in p. 214 of your xlixth volume); and in more than two-thirds of these instances, the differences, but little exceed *one-fourth of the unit*:—such are the very remarkable properties, and uses of this *Unit*, in defining and calculating Musical Intervals, for useful and practicable purposes.

By

By so contriving the parts of the Instrument intended for Temperament Experiments, that the connection of each of the 12 Finger-keys, could readily be shifted to different pipes, an artificial Comma apart, in succession, these important questions in Tuning, might at once be ascertained; viz. 1st, whether the same Person, in repeated tuning, can always produce the same identical Notes, throughout the 12?; and 2d, how far any two practical Tuners, agree in their results, in this respect?.

For insuring impartiality, and real experimental results in these trials, the tops and mouths of the Pipes, and all such parts of the Instrument might be kept closed and locked, which indicated, what the particular Notes were, which practical Tuners might thus select, by help of the blank sliders which they should have to move, while Tuning: and then, each One who should undertake to experiment in this way, might be allowed sufficient time, to deliberately and alone in a room, or only with such assistants as they might choose, in the best way satisfy themselves, which they could, as to every Note.

When this was done, a Committee of the Subscribers, or proper Persons appointed by them, should open the Instrument, and after writing down the 12 selected Notes, by means of the numbers of *artificial Commas*, (which would serve on all occasions as designations *for each of the individual Pipes* and Notes) the accuracy of *tune*, of each of such selected Notes, should be tried, by means of two, three or more of the Notes, above and below, which stand in relation thereto, as 6ths, 3rds, VIths and IIIrds, in preference to Vths or Octaves; because, small errors, are, by these less harmonious of the Concords, more strikingly shown and detected, than by means of the more harmonious of the Concords, Ists, VIIIths or Vths. Every result of such checks, on the tune of the Instrument, being noted down and preserved.

In this way, a great body of useful *facts* on the subject, might be collected; and perhaps, premiums might be offered among different practical Tuners, for such as could, in their Tuning, come the nearest in these trials, to any given System throughout; the Mean-Tone, the Isotonic, or Dr. Smith's Equal Harmony, for instance. And other Premiums, for those who could most exactly repeat their own particular mode of Tuning, in two or three successive Trials, &c. &c.

I am, sir, yours, &c.

37, Howland-street, Fitzroy-square, June 14, 1817. JOHN FAREY, Sen.

CII. *A*

CII. *A Method of communicating Rotatory Motion. By Chevalier* Baader, *First Counsellor of Mines, and Director-General of Hydraulic Constructions to His Majesty the King of Bavaria, &c. &c. &c.**

Sir, — I beg leave to offer to the Society of Arts the plan of a mechanical invention, which I have executed with success in my own country, but which is not yet known in this, nor published any where.

It is a method of communicating rotatory motion to a considerable distance from the person or persons who turn the machine, and without the use of wheels or pinions, band-wheels, or any other of the usual means. I made use of it in an hydraulic machine to raise water from the foundations of houses, &c., the workmen standing on the bank or firm ground, and giving motion to an axis with cranks placed over the water.

I flatter myself that this invention will be found so much more worthy of your celebrated and patriotic Society, as its utility is perfectly ascertained by the experience of a number of years in Germany; and, as its application may also prove beneficial to this country in many instances.

I am, with perfect esteem, sir,

Your most obedient humble servant,

No. 5. Salisbury-street, Strand, Nov. 8. 1816. Joseph de Baader, M.D,

To C. Taylor, M.D. Sec.

Description of the Machine. (See Plate V.)

The two levers, A B, turning on the centres, C D, of the moveable bars, E F, suspended on their centres, E F, attached to the frame of the machine, having a turning handle, G, which connects their ends, H I, and being attached by cranks to the wheel and axle, K L M, at their other extremities, communicate any rotatory motion given to the handle, G, to the wheel and axle, but in a reversed direction, as marked by the arrows, N O; and this, without the use of wheel-work, band-wheels, &c. as hitherto done.

In the drawing of the machine sent by the Chevalier de Baader to the Society, this contrivance was employed to communicate rotatory motion given to the handle, G, by labourers standing upon the platform, P Q, of the machine, on the bank of a reservoir, &c. from which water was to be raised, to the wheel and axle, K L M, placed over the water to work pumps, &c. as required.

* From *Transactions of the Society for the Encouragement of Arts, Manufactures and Commerce*, for the year 1816.—The Society's silver medal was voted to Chevalier Baader for this communication.

quired. It may also be employed in a great variety of other situations with equal convenience; as it is quite immaterial whether the levers, A B, are suspended from above, as in this instance; below, or even side-ways; and their points of suspension may likewise be varied, when necessary, by a suitable provision in their construction, so as to be placed either nearer to the wheel and axle, in order to act upon them by the handle, G, with greater leverage, to gain power; or further from them, to gain time; as the handle, G, then moving in a circle of lesser diameter, can consequently be turned more swiftly about.

CIII. *A new Modification of* NOOTH'S *Apparatus, and of* WELTER'S *Tube of Safety:—The Chrysoprase submitted to the Oxihydrogen Blow-pipe. Communicated by* J. MURRAY, *Esq.*

To Mr. Tilloch.

SIR,—HAVING found the apparatus, of which the drawing accompanying this is a correct portrait, a useful instrument in my own manipulations, and considering it a valuable simplification of Nooth's apparatus, as the safety-tube is an improvement of that of Welter,—Mr. Steele of this place, the projector, has been induced at my request to permit me to submit it to you; which I now do, in the conviction that it will be found an important acquisition to the chemist.

I am respectfully, sir, your most obedient servant,

Greenock, May 31, 1817. J. MURRAY.

P. S. I have submitted the *chrysoprase* to the flame of the oxihydrogen blow-pipe. This gem, which is silex coloured with oxide of nickel, has been among the most refractory substances which I have yet exposed to its energy. Soon after it became intensely red hot, it lost its colour and became *white* tinged with *yellow*. The edge of the polished crystal seemed *abraded*, and fractures were discoverable in the crystal; but fusion was not evident. J. M.

Description of STEELE'S *Modification of* NOOTH'S *Apparatus, and of* WELTER'S *Tube of Safety.*

A is a globular receiver, to contain the liquid required to be saturated with the gas arising from the retort B.

C is a quilled balloon, the quill of which is accurately ground into the neck of A, and nearly reaches its bottom.

Into the neck of the balloon, C, a safety appendage, D, is fitted; the bulb of which may contain a further portion of the liquid to be saturated (or mercury, if a greater pressure be required).

quired). The gas evolving from the retort presses on the surface of the liquid in A, and causes it to ascend through the quill into C, until the orifice of the tube being left open, it necessarily bubbles through the liquid; and what does not combine with it, passes through the fluid in the bulb of the safety-funnel, D; and raising the liquid into the upper cup, finally escapes. In the event of a vacuum in the retort and lower receiver, it is obvious that the air has free access through the communicating tubes of the safety-funnel and quilled balloon.

The impregnation is very soon effected, as the pressure is great, and as the several parts are fitted by accurate grinding. Much time is saved, and inconvenience, from the usual mode of luting being avoided.

CIV. *Improved Method of working a Capstan. By Lieut.* M. SHULDHAM, *R. N.* *

SIR, — I BEG leave to present to the Society a very simple contrivance of mine for applying, with much ease and dispatch, an additional power to a capstan when required. The method has been tried on board His Majesty's brig Cordelia, and was found to answer. Many naval officers have also approved it, deeming the contrivance a particularly useful one when applied to a ship's capstan.

I have the honour to be, sir,
Your obedient humble servant,
MOLYNEAUX SHULDHAM.

No. 49, Great Titchfield-street,
April 9, 1816.
To C. Taylor, M.D. Sec.

Copy of a Report of Lieut. George Eyre Powell, First Lieutenant of His Majesty's Brig Cordelia, relative to Lieut. SHULDHAM'S *Method of applying more Men to the Capstan.*

I have had an opportunity of trying your plan on the capstan; it answers in every respect as you stated. Indeed, it is too great a purchase; as, yesterday morning, against a strong gale and tide, I clapped on your swifter, and had the pleasure to carry away a seven-and-a-half-inch messenger: got the messenger removed to a fresh part, and carried it also away; so you may judge there was no foolish wind or tide. I was afterwards obliged to have recourse to a runner and pendant. The messenger

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures and Commerce*, for the year 1816.—The silver medal was voted for this communication, and a model of the capstan is preserved in the Society's repository.

senger was quite new when we left this place, and had not been used more than five or six times."

(Signed) G. E. Powell, First Lieut.
Sheerness, Feb. 9, 1816. *Brig Cordelia.*
To Lieut. Shuldham, &c.

Certificate.

I hereby certify, that I have seen Lieut. Shuldham's contrivance for the application of additional men to a capstan, when it is required to have a great strain, and have no doubt of its efficacy; and as it is not attended with any trouble or expense, I think the invention, when known, will certainly be generally adopted, its simplicity and utility being so obvious.

April 26, 1816. (Signed) A. Brown, Commander, R. N.

Reference to the Engraving of Lieut. Shuldham's *improved Method of working a Capstan.* Plate V. fig. 2.

In addition to the usual manner of placing men between the capstan bars, the ends of the bars should have notches or gaps made in them, in which an endless rope, A B, is received, and passes through two pulley blocks, C D; thus forming two straight lines, along which men being placed, can act by pulling the ropes in the most efficacious manner upon the capstan.

CV. *Observations on the peculiar Alternations in the Colour of Antares, or the bright Star in the Heart of the Scorpion, compared with that of other Stars. By* Thomas Forster, *Esq.*

Sir, — I have heretofore had occasion to notice the mutations in the colour of the light of some of the larger fixed stars, and have communicated such observations through the medium of your Magazine. I have now to commemorate the great, and, I may say, unusual brilliancy of this phænomenon observed last night in the star *Antares*, which has afforded an opportunity, during the serenity of a long summer's night, of minutely observing and defining it, together with the concomitant twinkling. About 9^h $35'$ I first noticed it. The star twinkled a great deal; and this motion called twinkling, seems to consist of successive dilatation and brightness, and of apparent contraction and dullness, alternating with each other. The alternating colours are deep red and bright white with a tinge of blue. To be more explicit, I have compared the red colour to the colour of copper filings ignited with gunpowder in the pyrotechnical Jerbs, and the white colour to ignited steel filings. The red colour happens in general just at, or a little before, the maximum of the

the twinkle, or bright apparent dilatation of the star. It seems to occur only once in the course of five or six dilatations, though now and then oftener: I could not distinctly ascertain any regular periods of it. The intervening colour of the light hardly varied, except in the brilliancy of the twinklings. Being at Tunbridge Wells, on very high ground, I watched the phænomenon during great part of the night: sometimes the red colour was very intense and bright, and was followed by an almost disappearance of the star by the apparent contraction. I do not know whether I use terms sufficiently explanatory; but it is difficult, in describing phænomena not much treated of before, to select the most appropriate terms. I hope I shall convey to the reader a tolerably clear idea of the fact, and that corresponding observations will be made. I noticed this appearance six or seven years ago, in the month of September; but it has certainly increased since that time. I may observe also that the twinkling of *Antares* is greater than that of any other star visible at the same time. As the above-mentioned alternation of colour is visible in several other stars, I have noticed them, with some of the peculiarities attending the changes of their light.

1. *Antares* in mean longitude $8^s\ 7^\circ\ 14'\ 24''$; mean south latitude $4^\circ\ 30'\ 48''$. Has the greatest twinkling, and most intense red colour alternating with white in the most irregular manner.
2. *Betalgeus*, or *α Orionis*, mean longitude $2^s\ 26^\circ\ 14'\ 26''$; south latitude $16^\circ\ 3'\ 50''$. Alternations of red and yellowish, not so distinctly marked as the last, and the times of the red bear a greater proportion to those of the yellow.
3. *Aldebaran*, or *α Tauri*, longitude $2^s\ 7^\circ\ 16'\ 26''$; south latitude $2^\circ\ 29'\ 16''$. Alternation still less conspicuous.

As the above stars are in very different places in the heavens, and as the stars about them do not show the least alternation; so I think the change of colour cannot depend on any thing in our atmosphere, but must attend some alteration going on in the alternating star. I must observe also, that this phænomenon is seen only in the red stars; it is faintly observable in *Arcturus*, but wholly imperceptible in *Lyra*, or the bright star in the *Eagle*. In *Syrius* it is faintly observable, but perhaps the least so of all. *Syrius* is reported by the old astronomers to have been a red star; and perhaps the alternation I allude to, may be somehow connected with the more permanent change of colour of varying stars. The phænomenon is seen with refracting (and probably also reflecting) telescopes, and certainly deserves the attention of astronomers. I am, sir, &c.

Spa Lodge, Tunbridge Wells, THOMAS FORSTER.
June 20, 1817.

P.S. I saw the phænomenon alluded to in the Journals, in *Leo*, in longitude $4^s\ 27° 21' 50''$. It appeared in the telescope just above *Regulus*, like a *small yellow star*.

CVI. *Notices respecting New Books.*

The Philosophical Transactions of the Royal Society of London for 1817, Part I. has just been published, and contains the following papers:

I. An Account of the Circulation of the Blood in the Class *Vermes* of Linnæus, and the Principle explained in which it differs from that in the higher Classes. By Sir Everard Home, Bart. V.P.R.S. — II. Observations on the *Hirudo vulgaris*. By James Rawlins Johnson, M.D. F.L.S. &c.—III. On the Effects of Galvanism in restoring the due Action of the Lungs. By A. P. Wilson Philip, Physician in Worcester.—IV. An Account of some Experiments on the *Torpedo Electrus*, at La Rochelle. By John T. Todd, Esq.—V. A Description of a Process by which Corn tainted with Must may be completely purified. By Charles Hatchett, Esq. F.R.S.—VI. Observations on an astringent Vegetable Substance from China. By William Thomas Brande, Esq. Sec. R.S.—VII. Some Researches on Flame. By Sir Humphry Davy, LL.D. F.R.S. V.P.R S. — VIII. Some new Experiments and Observations on the Combustion of gaseous Mixtures, with an Account of a Method of preserving a continued Light, in Mixtures of inflammable Gases and Air, without Flame. By Sir H. Davy, LL.D. &c. &c.—IX. De la Structure des Vaisseaux Anglais, considérée dans ses derniers Perfectionnements. Par Charles Dupin, Correspondant de l'Institut de France, &c.—X. On a new fulminating Platinum. By Edmund Davy, Esq. Professor of Chemistry, and Secretary to the Cork Institution.—XI. On the Parallax of the Fixed Stars. By John Pond, Esq. Astronomer Royal, F.R.S. Appendix to Mr. Pond's Paper on Parallax.—XII. An Account of some Fossil Remains of the Rhinoceros discovered by Mr. Whitley in a Cavern inclosed in the Limestone Rock from which he is forming the Break-water at Plymouth. By Sir Everard Home, Bart. V.P. R.S.—Meteorological Journal.

Sir John Sinclair's *Code of Agriculture.*

However doubtful or mysterious the art of Agriculture may have formerly been considered, yet by the various improvements which have been made in that art, and the great increase of knowledge which has of late years been amassed, the difficulties attending

attending the practice of an improved System of Husbandry have, in a considerable degree, been removed, and its principles have become so much simplified, and so well understood, that the time has at last arrived, when it is possible to undertake the arduous task of drawing up "*A Code of Agriculture.*"

Till the present period had arrived, this could not have been attempted with any well-founded hopes of success; for so many able and well-informed individuals had never, in any former era, directed their attention to agricultural pursuits;—so much capital had never previously been employed in the cultivation of the soil;—so many practical farmers had never before published the result of their experience, and observations on agricultural subjects; nor had those minute operations, on the due execution of which the success of the farmer must in a great measure depend, been ever formerly so distinctly pointed out.—Hence the superiority of the present era for such an undertaking.

And if such a work were to be attempted at this time, there is perhaps no individual on whom it was so incumbent to endeavour to prove that it might be executed, as the person who now ventures to offer the result of his labours to the public. On this suggestion, the Government of Great Britain established a Board of Agriculture, and Internal Improvement; under whose auspices the greatest exertions were made to collect useful information, as a foundation for such a work as the one now proposed, the publication of which, from the commencement of the new Institution, was in his contemplation. A great body of valuable materials having been thus amassed*, what could be more desirable than to reduce the substance of the whole, into so moderate a compass, that it would require neither much expense to purchase, nor much time to peruse? How far it is practicable to carry such an idea into effect, the reader will soon have an opportunity of judging.

But to enable any person to undertake such a task as the present, it was not alone sufficient that he had access to books, however numerous, or however valuable the information they might contain. It was necessary for him to converse with farmers, to discuss the various subjects connected with agriculture, with practical men; to survey their farms; to examine their various practices on the spot; to compare the systems of different

* For that purpose, the agricultural circumstances of every district in the kingdom were minutely examined, and Reports published, of the state of the several Counties of England, in 47 volumes octavo: and of Scotland, in 30 volumes more. Seven volumes of Communications, in quarto, and several other works on specific subjects, have also been published by the Board; and this national undertaking was completed at an expense of nearly two hundred thousand pounds.

 countries—

countries—and, above all, to be himself a farmer, and that on a great scale. These advantages have not been wanting on the present occasion.

After considering deliberately how the proposed plan could best be executed, the following appeared to him the most simple, and the most comprehensive that he could devise.

I. To consider those "*preliminary points*," to which a farmer ought to attend, otherwise he can never expect to carry on, in a successful manner, any system of husbandry. These particulars are,—Climate;—Soil;—Subsoil;—Elevation;—Aspect;—Situation;—Tenure, whether in property, or on lease;—Rent; Burdens on, and Size of the Farm.

II. To inquire into the nature of "*Those means of cultivation, which are essential to insure its success:* these are Capital;—Regular Accounts;—Arrangement of Agricultural Labour;—Farm Servants;—Labourers in Husbandry;—Live Stock;—Implements;—Agricultural Buildings;—Command of Water;—Divisions of Fields;—and Farm Roads.

III. To point out "*The various modes of improving Land*," by Cultivating Wastes;—Inclosing;—Draining;—Manuring;—Paring and Burning;—Fallowing;—Weeding;—Irrigation;—Flooding;—Warping;—Embanking;—and Planting.

IV. To explain "*The various modes of occupying Land*," in Arable Culture;—Grass;—Woods;—Gardens;—and Orchards;—and,

V. To offer some general remarks on "*The means of improving a country*." By diffusing Information;—By removing Obstacles to Improvement; and,—By positive Encouragement.

This work is intended to form one large volume octavo (and will be published early in August), in the body of which, general principles can alone be dwelt upon. Where particular information is necessary, it will be inserted in notes; and where the subjects are of great importance, and require minute details, it is proposed to consider them in separate dissertations.

An Inquiry into the Abuses of the Chartered Schools in Ireland, with Remarks on the Education of the Lower Classes in that Country. 8vo.

An Essay on the Nature of Light, Heat, and Electricity. By C. C. Bompass, Barrister at Law. 8vo.

Remarks on Insanity, tending to illustrate the Physical Symptoms and Medical Treatment of the Disease. By Thomas Mayo, B. M.

The 1st number of a new Quarterly Journal has just appeared, entitled, The Continental Medical Repository: exhibiting a concise view of the latest discoveries and improvements made on the

Continent

Continent in Medicine, Surgery, and Pharmacy, conducted by E. von Embden, assisted by other gentlemen of the Faculty.

The third volume of the Zoological Miscellany will be published in September, illustrated with fifty-nine coloured plates.

The long-expected work of Dr. Spurzheim on Insanity has at length issued from the press. It contains some excellent and perfectly new observations on this hitherto obscure disease. But perhaps the most important part of the work is that which relates to the *confinement of persons suspected of insanity, on the part of their relations, &c. by the mere declaration of an ignorant practitioner of medicine that the patient is mad.* The author also takes particular notice of the *neglected state of madhouses, and of the hardships suffered by the insane in lunatic asylums*, and proposes many useful plans for their improvement. The principal doctrine maintained by the author is, that the diseases of the mind are always diseases of the brain, which is the organ of the manifestations of the mind in this its worldly state; and that they should be treated on the same pathological principles as other disorders of material parts. In the preliminary observations, are some very useful remarks on the distinction always to be made between the *suppression, the fatigue, and the exhaustion of the vital power;* and a reference is made to a curious work of Dr. Gall's, on diseases, which would be a very useful work if more known, and translated into our language.

In the press, The Principles of Diagnosis, by Marshall Hall, M.D. &c. This work is founded entirely on the external appearances of morbid affections. It embraces, 1. A view of the countenance and attitude of patients, inasmuch as they are plainly characteristic of diseases. 2. The symptoms of diseases, considered in their modifications, and in relation to particular affections. 3. A diagnostic arrangement of diseases; and, lastly, their diagnosis.

A Part of this work will be published in July.

The Third Volume of Mr. John Farey's Mineralogical and Agricultural Survey of Derbyshire will be published early in July.

This volume completes the Survey of that interesting County, which was made by Order of the Board of Agriculture, and contains a full Account of the Surfaces, Hills, Valleys, Rivers, Rocks, Caverns, Strata, Soils, Minerals, Mines, Collieries, Mining Processes, &c. &c. Together with some Account of the recent Discoveries respecting the Stratification of England; and a Theory of Faults and Denudated Strata, applicable to Mineral Surveying and Mining.

John Harrison Curtis, esq. Aurist to His Royal Highness the Prince Regent, and Surgeon to the Royal Dispensary for the *Diseases of the Ear*, is about to publish A Treatise on the Physiology and Diseases of the Ear, containing a comparative View of its Structure and Functions—and of its various Diseases. This Work is intended chiefly for deaf persons, and will be accompanied with an interesting Copper-plate, representing an invention of an Artificial Ear, made in France, and which very much increases the collection of Sound; but Mr. Curtis has made considerable improvement in this invention, which occasions the sound to enter with double force, by its being applied over the natural Ear.

Mr. Thomas Forster has in the press, and will shortly publish, A Monograph of the genus *Hirundo*, which will contain a collection of the evidence hitherto collected for and against the question of the migration of the swallow tribe; with numerous anecdotes. It will be illustrated by figures of the four British species.

I am happy in being able at length to announce the speedy publication of the First Part of the very useful Set of Arithmetical Tables, by Henry Goodwyn, esq. which were first introduced to the notice of my readers, in a communication from his friend Mr. Farey sen.*, inserted p. 385 of the xlviith volume. Every one used to calculations must have experienced the labour attending the reduction of vulgar fractions to their equivalent decimal fractions, although the denominators may be small, as under 100, if *many* places of figures are wanted; and the danger there is of making mistakes in such cases. And few persons are much versed in figures, without having very frequently seen the still greater labour and difficulties attending the reverse of the above process; viz. the reducing of a decimal fraction to its equivalent vulgar fraction, when such be correctly practicable, or otherwise, of finding *the nearest* vulgar fraction thereto ex-

* I beg here, to correct an inaccuracy and over-sight which occurred last month, in the hasty and very compressed abstract which, for want of room, I was obliged to give in p. 345, of the Labours of the Mathematical Class of the Institute of France; wherein the name of Mr. Farey follows that of M. Laplace, by mistake, instead of the name of *M. Cauchy*, whom the Secretary to the Institute mentions, as having given a general demonstration of the curious property of vulgar Fractions, which Mr. Farey had published in the Philosophical Magazine. Mr. Farey's late Communication to me on the subject, which I have given in a Note in the page quoted, correctly refers to what had appeared in the "*Analyse des Travaux*," &c. with respect to himself, and the error mentioned, lay entirely with me:—In line 7 from the bottom of the Note referred to, for *any*, read *my*.—Editor.

pressed

pressed in small numbers; as for instance, less than 100, in either of its terms. Both these operations, Mr. Goodwyn's Tables are calculated to perform by mere inspection; and in the latter particular, especially, to save much time to calculators, as well as secure greater accuracy to their results.

But Mr. Goodwyn goes a great deal further, as the title of his intended work imports; viz. a "First Centenary of a Series of complete Decimal Quotients;" because, this Table will admit, the taking out by inspection of the *complete value* in each case of every fraction between $\frac{1}{99}$ and $\frac{98}{99}$; that is, with as many places of decimals, as is requisite, either for the same terminating or circulating, as the case may be: showing all the various *decimal circles* belonging to each division under 1025.

The elaborate Tables of the same kind, intended to follow, if the present intended ones are favourably received, will carry Mr. Goodwyn's series of complete decimal quotients, to $\frac{998}{999}$; or even to $\frac{1023}{1024}$, the present extent, as I understand, of his truly laborious and curious calculations. An Introduction is intended to explain and show examples, of all the various uses of the Tables.

CVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 5. A PAPER by Dr. Leach, of the British Museum, was read, containing some observations on a new genus of marine animals inhabiting the argonaut and nautilus shells. It was observed by Sir Joseph Banks, that the animal found in these shells is not the fabricator of them, but a parasite which has taken up its occasional abode there when it chooses to shield itself from the direct action of the waves. This ingenious opinion has been promulgated several years; but no direct confirmation, or grounds for rejecting it, had hitherto fallen under the observation of naturalists. The late expedition to the Congo river has enabled Dr. L. to verify the truth of the learned President's observation, and furnished him with an opportunity of examining minutely one of these animals, and ascertaining to what genus it properly belongs. He described the means by which the animal attaches itself to the empty argonaut shell, the manner it leaves and returns to it, and the organs necessary for such migrations.

Sir E. Home, bart. also presented a paper somewhat similar, detailing his remarks on the mode and period of generation of the animals found in nautilus and argonaut shells. He found them to be oviparous animals, to be nourished nearly like snails; and,

and, what removes all doubt respecting the origin of the shells, he was so fortunate as to discover some of the animal's eggs on their margin, which satisfied him that they could not be the makers of their actual habitation. He then detailed the natural history of snails, whose progressive changes he considered as exactly corresponding to those of the animals in question; he observed that the eggs of snails, which are found in little white clusters, each egg being about the size of a common pin's head, and well known to gardeners, are altogether about 24 days in changing from the state of eggs till the perfectly-formed snail can creep from its nest. His description of the eggs and the yolks both of snails and the animal in the nautilus, showed their great similarity; and he concluded that it satisfactorily demonstrated the justness of the President's previous observations on these shells.

June 12. 19. Sir Wm. Herschel communicated a paper on the system of the scattering of the stars, and the best mode of dividing them into classes, so as to form a correct and convenient catalogue. The reading of this paper occupied the attention of the Society both these evenings.

The President and Council have adjudged the Gold and Silver Medals on Count Rumford's Donation to Sir H. Davy for his Papers on Combustion and Flame, published in the last Volume of the Philosophical Transactions.

ROYAL MEDICAL SOCIETY OF EDINBURGH.

The Royal Medical Society propose as the subject of a Prize Essay, the following question:

"What changes are produced on atmospherical air by the action of the skin of the living human body?"

The members only are invited as candidates. The dissertations are to be written in English, French or Latin, and to be delivered to the Secretary on or before the 1st day of December next. To each dissertation must be prefixed a motto, and this motto is to be written on the outside of a sealed packet containing the name and address of the author.

PHILOSOPHICAL SOCIETY, LONDON.

The Anniversay Meeting of the Philosophical Society of London was held at the Society's Rooms adjoining Scots Corporation Hall, Crane Court, Fleet Street, on Thursday the 12th of June. The following noblemen and gentlemen were chosen Officers and Council for the ensuing year.

President,

Right Hon. The Earl of Carysfort, K.P. F.R.S. F.A.S. D.C.L.

Vice-

Vice-Presidents.

Right Hon. Lord Henniker, F.R.S. F.A.S.
Sir J. C. Hippisley, Bart. M.P. LL.D. F.R.S. F.A.S.
Isaac Hawkins Browne, F.R.S.
Rev. W. B. Collyer, D.D. F.A.S.
Olinthus Gregory, LL.D.
Rev. Abraham Rees, D.D. F.R.S. F.L.S.
James Sowerby, F.L.S. G.S. W.S.
J. F. Vandercom, F.G.S.

Treasurer and Honorary Secretary,
Thomas Joseph Pettigrew, F.L.S.

Registrar,
John Miers. *Assistant ditto,* T. R. Cromwell.

Curators,
W. C. Pettigrew; T. J. Arniger.

Orator for 1818.
John Mason Good, F.R.S.

Council.

Thomas Adams; James Andrews; Jonathan Barber; Rev. Geo. Bathie, D.D.; Thomas Bedder; Benjamin Bensley; Clarke Burton; John Thomas Cooper; Geo. Dudley; Thomas Fisher; Charles F. Forbes, M.D.; H. C. Hodge; Samuel Meadows; B. H. Smart; Peter Thomas; Richard Thompson; Thomas Tucker; Rev. T. M. Young, LL.B.

The Anniversary Oration was delivered by Dr. Gregory, and will shortly be published. It was very numerously attended, as was also the dinner; and many excellent addresses made by His Royal Highness the Duke of Sussex, who was in the chair; by Lords Erskine, Henniker, &c.; Drs. Gregory, Mason, Collyer; Messrs. Coleridge, Pettigrew, &c. &c. &c. A volume of Transactions of the Society is now in the press, and will appear about the close of the present year.

CALEDONIAN HORTICULTURAL SOCIETY.

At a General Meeting of this Society held on the 10th June, the following communications were read to the meeting, and excited considerable interest:

1. On the Maturation of different Kinds of Fruit by artificial Heat, after Removal from the Tree; by James Howison, esq., Douglas.

2. Account of a new Variety of early Potatoe called the Kilspindie Bloom; by the Rev. Dr. Don.

3. Account of a simple Method of ventilating Hot-beds with Air moderately heated; by Alexander Keith, esq., of Ravelstone.

4. On

4. On the best Method of pitting and planting Trees on bare moorish Ground; by Mr. John Young, of Perth.

5. On the Advantages of removing the hard and scaly outer Bark of Fruit Trees, &c.; by Mr. Thomas Thomson.

Mr. Jardine, one of the Vice-Presidents, announced, that he had received from Dr. David Whyte, of Bombay, a packet containing seeds of some curious and rare plants to be presented to the Society. It was agreed that the seeds should be transmitted to the Royal Botanic Garden, and the thanks of the Society communicated to Dr. Whyte.

FRENCH ROYAL ACADEMY OF SCIENCES.

Sitting of 24*th March* 1817.

M. Desfontaines read a very ample Report upon a memoir of M. Desvaux, intituled *Dispositio methodica Generum Lycopodiorum et Filicum.*

The work of M. Desvaux furnishes us with some new genera, and one hundred and ten species of ferns not hitherto described. Their characters are exposed with exactness, and accompanied with designs. The author adds some observations upon many other ferns already known, and critical notes on their synonyms.

MM. Ampere and Cauchy made a Report upon a memoir of M. de Maugold, "On the Manner of calculating Interest."

It is known that when we take the interest of 5 per cent. at the end of a year as the product of a compound interest, the capitals increase not in arithmetical but in geometrical progression; so that, for example, to obtain the sum of capital at the end of six months, it is necessary, not to add to the original capital $2\frac{1}{2}$ per cent. but to multiply that capital by the square-root of 1 + 1-20th, which gives a result somewhat less than by the other process, the geometric mean between two quantities being always less than the arithmetic mean. The tables which M. Maugold has constructed are intended to facilitate the calculation of compound interest for any given number of days, and in the opinion of the reporters they will be found of great utility.

March 31.—M. Burckardt made a Report upon the New Orreries of M. Jambon.

The one which has been presented to the Academy is very complete. It represents the motions both of the old and the new planets—the diurnal movement of the earth—the parallelism of its axis of rotation—and the nodes of the lunar orbit. The reporters expressed themselves highly satisfied with it.

SOCIETY

SOCIETY FOR THE ENCOURAGEMENT OF INDUSTRY IN FRANCE.

General Meeting, 9th April 1817.

The meeting having been opened under the presidency of Count Chaptal, the Secretary Baron Gerando proceeded to read a Report of the labours of the Society during the preceding year.

In the department of experiments and observations, notice is taken of a siphon presented to the Society by M. Landren, which has two branches that convey at the same time both water and air, and is supposed by the inventor to be capable of renewing the air in mines. The committee of the Society to whom it was remitted, had not been able to form a judgement of this instrument, but from very imperfect models, and from reports the results of which they have not been able to verify. Similar in some respect to the tinman's pump of Seville, and the horns of the Catalonian forges, it can introduce air into furnaces and mines at all times when there is an opportunity of carrying off the water employed or deposited; but in the one case the humid air unavoidable by this method, must in the opinion of the committee be injurious to the fusion of the metals; and in the other case the chance they think is greater, of the noxious gases common to mines being aspired, than of their being displaced by the introduction of new air.

Among new improvements of existing processes, the attention of the Society was particularly directed to the perfection to which the preparation of platinum had been brought. Not only is the mode of purifying it most complete; but little ductile as it seems, it is now reduced into leaves as fine as those of gold. MM. Cuog and Coutourier of Paris have presented to the Society a vase of platinum, purified according to the process of M. Breant, assayer to the Mint, which is formed of one single leaf without soldering, contains 160 litres, and weighs 15½ kilogrammes (31 lbs.). The cost is 18 francs per ounce. The vase is intended to be employed in the concentration of sulphuric acid. It is but just, the Report adds, to observe that Janety the younger was the first to fabricate vases of platinum of a large size, but not without soldering. This artist furnishes the metal at present at 14 francs the ounce, either in plate or wire.

The most remarkable of the new inventions which have been submitted to the Society is one of a portable anemometer, constructed by M. Regnier. The idea of it was suggested to the inventor by M. Buffon. It has been applied in a very ingenious manner to make a hall clock indicate not only the force and direction of the wind, but even the maximum of action which it has exerted during the absence of the observer.

CVIII. *In-*

CVIII. *Intelligence and Miscellaneous Articles.*

HYPERSTHENE ROCK.

Dr. MacCulloch has given this name to a new rock which he has discovered in Scotland. He has found it in two places; in the island of Sky, and in Airdnamurchan. In the former it constitutes the great and picturesque mass of mountains known by the name of the Culin hills; but is not limited to them, as Dr. M. has found that the hills which lie immediately to the east of these, and which encircle the romantic valley of Coruisk, are formed of the same rock: he even suspects that a portion of Blaven also is composed of it. Dr. M. ascertained the existence of this rock five years ago, pointing out to travellers at the same time the picturesque and sublime scenery of the spot where it is found, at that time unknown out of the island, and scarcely indeed known to any within it, except the proprietor Mr. Macalister of Strathaird and his shepherds. Our readers are doubtless acquainted with the popular poem, "The Lord of the Isles," in which it is described; the curiosity of the poet having, fortunately for the public, been excited in consequence of that representation. But the descriptive powers even of Walter Scott fail to convey an idea of that which exceeds as far the powers of poetry as it does those of painting. Loch Scairg must be seen to be felt; nor will the traveller repent his labours, if the only reward of his Highland tour should be a day spent among the wild mountains that surround this romantic spot. We shall render both the traveller and the mineralogist an acceptable service, by informing them of a route not described by the Scottish tour-books, and by facilitating their movements in a country but little known—fully as little to the lowlanders beyond Cheviot, as to us southerns.

There is an excellent road of forty miles from Fort William to Arasaik, whence there is a ferry to Ardavasar in Sky. Roads that can be walked, or traversed on a Highland pony, lead from here to Kilbride, Tarskairg, Gillan, Ord, and other *towns* on the west side of Sleat, whence boats can be procured, in which the voyage to Loch Scairg may be completed in a day; or the traveller may set up his quarters at Broadford, and thence make his excursion with perhaps greater facility. It is not accessible by land.

The mountains composed of this rock are remarkable for their rugged outlines and dark colour; from which causes they are distinguishable far at sea, being strongly contrasted with the neighbouring hills. They present the general features of granite, the rocks being disposed in large continuous masses with curved surfaces, and sometimes appearing to form large concretions of an oblate

oblate spheroidal shape. This rock is singularly durable; seeming to undergo no waste capable of generating a soil, either upon the surface or at the feet of the mountains. Hence they present an aspect of entire desolation, their whole visible declivity, where they surround the lake, being brown and bare, with scarcely a mark of vegetable life.

The geological relations of this rock are ascertained, not only by its intimate connexion with the greenstone, basalt, amygdaloid, and sienite, that form the greatest part of the island, but by its actual overlying position on beds of red sandstone, which are fully described in Dr. M.'s papers before the Geological Society, from which we have extracted these circumstances. Hypersthene rock is therefore a member of the trap family; and we think there is reason to conclude that it is also of later origin than the lias formation—a reason derived from the author's account of the other parts of the island.

There are many varieties of structure and appearance; but the varieties of composition seem reducible to three: hypersthene with greenish compact felspar, with dark purple ditto, and with crystallized felspar approaching to glassy. The concretions vary much in size: when large, the nature of the rock is easily ascertained; when minute, it is easily mistaken for greenstone; since the hypersthene may be confounded with hornblende, a mistake which the author suspects to have been often made. The purple felspar resembles that of Labrador, but is never iridescent. Contemporaneous veins traverse the rocks in many places, containing very large concretions, and affording fine specimens of the several mineral substances. It must also be added, that veins of oxidulous iron ore are found in it; and that crystals of this substance of a tetrahedral form constitute in many cases an integrant part of the rock.

It is probable that a similar rock is the birth-place of the hypersthene and purple felspar of Labrador, and that it will be found hereafter in many places where it has either been overlooked, or confounded with common greenstone. It has generally happened in the history of mineralogical discovery, that a substance has been found common when once it had been shown to exist. We may instance the quartz rock of the same author, long overlooked as an occasional substance of trifling extent, or confounded with mica slate, and now proved to form a leading division of the primitive rocks all over the globe, since it occurs in the mountainous regions of India and southern Africa, occupying tracts of great extent.

A proof of this is given in the author's discovery of the same Hypersthene rock in Airdnamurchan, a spot frequently examined by geologists. Here, as in Sky, it forms an extensive tract, hi-

 therto

therto mistaken for greenstone. This place is of easy access, both by sea from Mull, and from the interior by means of the Moidart and Airdnamurchan new roads.

We may terminate these remarks by suggesting to mineralogists the necessity of examining more minutely into the supposed greenstones of the trap family. There is reason to expect that among these AUGIT ROCK, also described in the same memoir, at Sky, will be found to occupy the place too often assigned to greenstone. Mineralogists have for some time remarked, that augit was found in trap rocks; but the author above mentioned has found it so prevalent, where formerly overlooked, or confounded with the greenstones containing hornblende, that he has been induced to designate this member of the trap family also by an appropriate term. It forms a large tract on the north-east coast of Sky; and it seems moreover to constitute nearly the whole trap formation of Rum; besides which, we understand that he has traced it in many other parts of Scotland. It is composed of augit and felspar, both of these minerals being subject to considerable variations, and the compounds therefore presenting a great variety of aspects. When the augit is green and the felspar glassy, as in some parts of Rum, the rock is easily recognised; when the former is black, and the latter of the compact kind, it is more difficult to distinguish it from ordinary greenstone. When extremely fine, it forms a rock not to be distinguished from basalt by mere inspection. We expect a fuller account of this important rock from the author, and of the way in which it can, under all its aspects, be distinguished from common greenstone. The utility of the name will be apparent to all who know the power that names possess in natural history of directing attention to the objects of its pursuit; and there may also be pointed out an important and new analogy thus proved to exist between the traps and the lavas of modern volcanoes, of which augit, and not hornblende, has also been recently shown to be a principal constituent.

CHARCOAL-FIRE.

Notwithstanding the numerous accidents arising from burning charcoal in close rooms, a correspondent assures us, that he, as well as several of his friends, to whom he has recommended it, has experienced almost immediate relief from cough and catarrhal affections by sitting a few hours in his library with a chaffing-dish of burning charcoal near his feet. He has found this practice so effectual a check to the effects of cold during the winter season, that he can assuage even a violent catarrhal cough in the course of a single day. It has even relieved persons with weak lungs, and who are consequently subject to coughs during the continuance of cold weather or easterly winds.

MR.

MR. PLAYER'S STOVE.

To Mr. Tilloch.

Sir,—In that part of my paper describing the construction of an œconomical culinary stove, from which the extract relative thereto was made, which appeared in your last Number, I observe that I did not make myself sufficiently intelligible with respect to the direction of the flue which surrounds the oven. After it has passed under the bottom, it rises and expands over the side furthest from the fire-place, and thence passes vertically round the back to the other side without ascending or descending. For the channel, being two inches and a half or not more than three inches wide, necessarily occupies nearly or quite the whole height of an oven of twelve inches in passing round the angles at its back, as the section of the flue should not be much less than thirty square inches in any part of its course to ensure what is called a good draught.

I fear I was likewise somewhat obscure in describing the situations of the moveable stoppers, but these an intelligent workman will readily adapt to the flues. One may be placed in the side furthest from the fire-place at the bottom of the back angle where the flue ascends from under the oven over its side, and passes round its back, for the convenience of cleaning these different parts; another may open into the flue on the side of the oven next the fire-place, at the bottom of the angle where it passes from the back round that side; the third over the oven-door.

In submitting these remarks, I beg pardon for intruding so much on your attention.

I am, sir, your obedient servant,

Malmsbury, June 7, 1817. Richard P. Player.

We are concerned to state the death of Richard Lovell Edgeworth, esq. at Edgeworth's Town, Ireland, on Friday the 13th of June, at the age of 74. He had been complaining for some time, but was not thought to be in danger till within two days of his death.

LIST OF PATENTS FOR NEW INVENTIONS.

To Philip Hutchinson Clay, of London, for his new combination of machinery for the purpose of repairing and improving turnpike and other roads and highways, and preserving the same in good order.—2 months allowed for lodging his specification, dated 22d May 1817.

To

To Seth Hunt, of the United States of America, now residing in Covent Garden, for a new improved escapement for clocks and watches and chronometers, communicated to him by a certain foreigner residing abroad.—22d May.—6 months.

To Roger Didot, formerly a paper manufacturer in France, but now of Paddington, for certain improvements upon the machines already in use for making wove and laid paper in continued lengths or separate sheets.—22d May.—6 months.

To George Manwaring, of Marsh Place, Lambeth, for improvements in steam-engines.—22d May.—6 months.

To Seth Hunt, of the United States of America, but now residing in Covent Garden, for certain improvements in machinery for making pins, communicated to him by a certain foreigner residing abroad.—23d May.—6 months.

To Charles Wyatt, of Bedford-row, coppersmith, for his new method or methods for preventing any disadvantageous accumulation of heat in manufacturing sugar.—3d June.—2 months.

To Benjamin Ager Day, of Birmingham, for certain improvements in chimney ornaments, which said chimney ornaments are so constructed, that they may be used for fire-screens, flower sweet jars, time-piece cases, candlesticks, toast-stands, and various other purposes.—3d June.—2 months.

To Gabriel Tigere, Duke's Court, Bow-street, for his process or method of manufacturing writing paper, in such a manner as that it will be extremely difficult if not impossible afterwards to extract or discharge any writing from such paper.—3d June.—6 months.

To John Parnall, of St. Austell, Cornwall, for his method of tinning, or covering with tin, sheets or plates of copper, brass, or tink.—10th June.—2 months.

To Thomas Whittle, and George Eyton, both of Chester, for their improved kiln for the purpose of drying malt, wheat, oats, barley, peas, beans, and other substances, by means of steam assisted by air.—10th June.—6 months.

Astronomical Phænomena, July 1817.

D. H. M.		D. H. M.	
1. 9.53	☾ ε ♑	22.19.22	☉ enters ♌
7. 0. 1	☾ ♂	22. 0. 9	α ♎
8.14. 4	☾ in apogee	23. 0. 0	Moon in perigee
8.14. 6	☽ 125 ♉	24.13.26	☽ θ Ophiuchi
14. 7.47	☽ 132 ♉	25.21.20	☽ φ ♐
16.11.14	☽ η ♌	26. 1.36	☽ σ ♐
18. 9. 9	☽ ν ♍	28.19. 2	☽ ε ♑

Meteoro-

Meteorological Observations kept at Walthamstow, Essex, from May 15 *to June* 15, 1817.

[Usually between the Hours of Seven and Nine A.M. and the Thermometer (a second time) between One and Two P.M.]

Date.	Therm.	Barom.	Wind.
May			
15	47 59	29·76	SE—SW.—Sun and clouds; fine warm day; a shower after 5 P.M.; star-light.
16	46 64	29·98	SW—SE.—Sun and hazy; very fine day; star-light. New moon.
17	47 65	29·98	SE.—Cloudy and *stratus;* shower 8 A.M.; fine day; star-light; *stratus* NW.
18	57 59	29·70	SE—NE.—Clear and *cumuli;* fine day; rain from 3 P.M. to 5 P.M.; cloudy.
19	46 54	29·50	NE—E.—Cloudy and windy; gray day; 4 P.M. rain began; very rainy evening.
20	44 53	29·54	NE—E.NE.—Rainy, till about 10 A.M.; showers, sun and wind; frequent rain all day; set rain.
21	45 47	29·43	NE—W.NW.—Rainy; 1 P.M. rain; rain continued all day till 7 P.M., then sun and clouds and wind; moon in a *corona,* and clouds.
22	43 55	29·44	W.—Cloudy; clouds; sun and wind; slight rain at 8 P.M.; clouds; clear and moon.
23	47 60	29·44	NW—SW.—Clouds; clear and wind; very fine day; about 4 P.M distant thunder; clear night; slight *cirrostratus* NW.
24	44 64	29·49	NW—S.—Sun, and *cirrostratus* NW; *cumulostratus* and sun; slight showers after 2 PM; clear moon- and star-light. First quarter.
25	52 53	29·33	E—E.—Great showers and sun; 11 A.M. great shower; hail and rain; very showery day; fine clear evening.
26	52 62	29·22	E.SE.—Clear and clouds; slight showers; fine day; *cirrocumuli* and clear.
27	55 61	29·44	E.SE—SE.—Slight rain and clear; wind and showers; fine day; moon- and star-light.
28	52 59	29·65	S—NE.—Hazy and sun; clouds and some sun; showers; rain.
29	49 48	29·67	NE—N.—Rainy; great showers and wind; very stormy day; cloudy.
30	47 53	29·78	NE.—Wind and *cirrostratus;* gleams of sun; fine day; clouds and clear. Full moon.
31	44 58	29·88	NE—NW.—Clear and clouds; *cumuli,* and wind; clouds and sun; fine day; star-light.

June			
1	47 61	29·78	NW—W.—Bright sun; *stratus* NW; fine day; cloudy.
2	57 54	29·73	SW—W—S—SW—SE.—Sun and *cirrostratus;* black *cirrocumulostratus,* and some drops of rain; cloudy and windy.
3	49 54	29·64	W—SW—S.—Clear sun; fine day; rainy evening.
4	54 65	29·53	SE—SW.S—SW.—*Cirrostratus;* sun, and some showers; stormy wind; windy, and thin *cirrostratus;* and stormy wind and stars.
5	52 66	30·00	W—SW—SE—W.SW.—Sun and wind, and *cirrostratus;* clouds and wind; fine day; clear star-light.
6	56 66	30·00	S.—Wind and *cirrostratus;* gleams of sun; star-light. Moon last quarter.
7	60 67	30·00	SW—S.—Wind and clouds; very fine day; *cirrus* and clear; *cirrocumulostratus* and *cumuli,* and clear.
8	60 66	29·77	W—S.—Clear, and *cumuli;* fine day till after 4 P.M., then great showers; clear and *cumulostratus.*
9	60 57	29·78	NW—S.—Rain; very rainy day; rainy.
10	55 66	29·87	SE—NW—N.NE.—Slight showers; showers and sun, and windy; star-light.
11	51 58	29·99	NW—W—SW.—Clear; very fine warm day; cloudy.
12	56 58	29·78	SE—E—W.—Damp and rainy, and windy till near 2 P.M.; showers; *cirrostratus* and clear.
13	52 66	29·66	SE—S.—Cloudy early; rainy day till after 5 P.M.; *cumuli,* and clear and windy.
14	54 64	29·48	S—W.—Clear, clouds, and windy; showers, and sun; clear. New moon.
15	51 64	30·22	NW.—Sun and *cumuli;* very fine day; fine clear evening; *stratus* NW.

Errata in last month's Magazine:

May			
7	43 59	30·20	NE.—Sunny and hot; fine day; star-light.
8	46 72	29·87	W.—Hot and sunny, and *stratus* NW; *cumulostratus, cirrocumulostratus, stratus, cumuli, cirrostratus,* and a storm of rain at 4 P.M.; and distant thunder; star-light night.

This error was probably made in copying from the original journal, as the Thermometer, Barometer, and Wind were stated *right* on both days, and the weather *wrong.*

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1817.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
May 14	28	55·	29·75	Fair—heavy A.M.—thunder storm with hail P.M.
15	29	57·	29·92	Ditto—heavy shower P.M.
16	new	60·	30·05	Ditto
17	1	64·	29·94	Very fine
18	2	61·	29·70	Showery—rain P.M.
19	3	54·	29·67	Fine
20	4	48.5	29.67	Showery
21	5	46·	29·57	Rain
22	6	50·	29·58	Fair—rain in the morning
23	7	53·	29·58	Ditto
24	8	62·	29·63	Very fine
25	9	52·	29·51	Rain—very heavy rain P.M., and at night
26	10	62·	29·45	Very fine
27	11	64·5	29·65	Ditto
28	12	54·5	29·79	Fair
29	13	48·	29·83	Cloudy—rain A.M.
30	full	50·	30·	Ditto
31	15	52·	29·95	Fair
June 1	16	61·	29·85	Very fine
2	17	61·5	29·83	Fair—cloudy
3	18	62·	29·76	Fine
4	19	58·	29·63	Cloudy
5	20	63·	30·10	Fine
6	21	66·	30·07	Very fine
7	22	71·5	29·95	Ditto—cloudy. The thermometer stood higher this day than any day in 1816.
8	23	63·	29·80	Fine
9	24	57·5	29·85	Rain
10	25	56·	29·88	Ditto
11	26	63·	30·10	Very fine
12	27	58·	29·81	Rain
13	28	55·5	29·60	Rain
14	new	53·	29·52	Rain till P.M.—showery. The wind has been high these three days, varying from all points of the compass.

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For June 1817.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
May	27	51	60	50	29.48	30	Cloudy
	28	52	55	45	·65	26	Showery
	29	45	47	45	·72	0	Rain
	30	46	53	45	·80	32	Cloudy
	31	47	55	47	·75	46	Cloudy
June	1	47	59	50	·70	48	Showery
	2	52	60	52	·65	47	Showery
	3	55	64	50	·68	55	Fair
	4	51	60	51	·57	52	Stormy
	5	50	63	55	30·02	45	Cloudy
	6	57	68	57	29·99	42	Cloudy
	7	62	72	60	·80	55	Fair
	8	55	60	54	·75	42	Stormy
	9	55	58	55	·76	0	Rain
	10	55	65	54	·80	57	Stormy
	11	56	65	55	·95	56	Fair
	12	56	60	55	·70	0	Rain
	13	55	64	55	·32	0	Rain
	14	56	62	52	·60	36	Stormy
	15	54	64	55	30·10	42	Cloudy
	16	55	67	55	·02	57	Fair
	17	55	69	57	29·89	62	Fair
	18	62	75	66	·62	55	Fair
	19	69	79	69	·66	66	Fair
	20	70	83	69	·72	84	Fair
	21	69	83	69	·84	85	Fair
	22	67	84	67	·98	87	Fair
	23	67	80	64	·90	82	Fair
	24	66	80	70	·85	72	Fair
	25	65	78	64	·90	55	Fair
	26	61	75	69	·73	62	Fair

N.B. The Barometer's height is taken at one o'clock.

INDEX TO VOL. XLIX.

END OF THE FORTY-NINTH VOLUME.

Printed by R. and A. Taylor, Shoe-Lane, London.

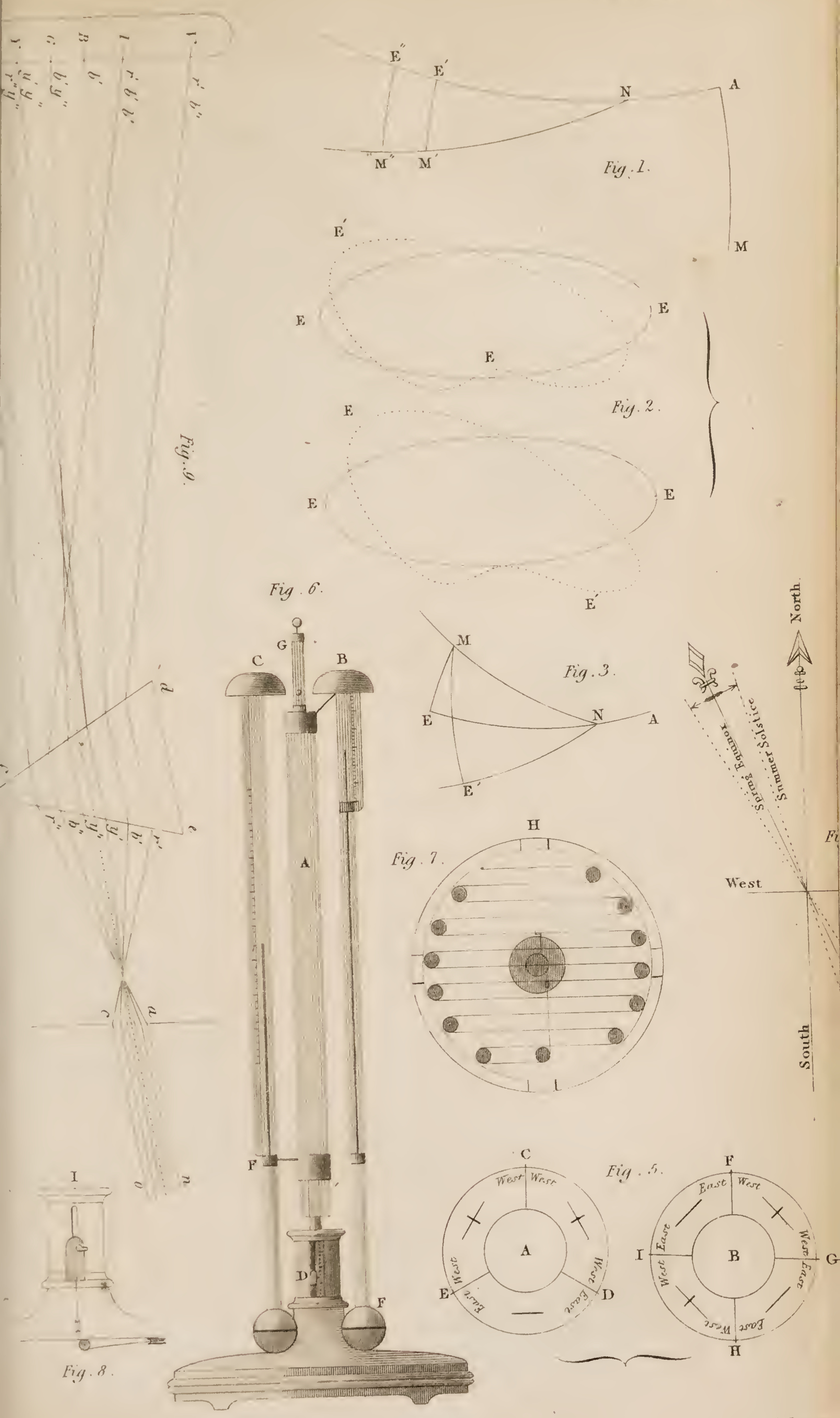
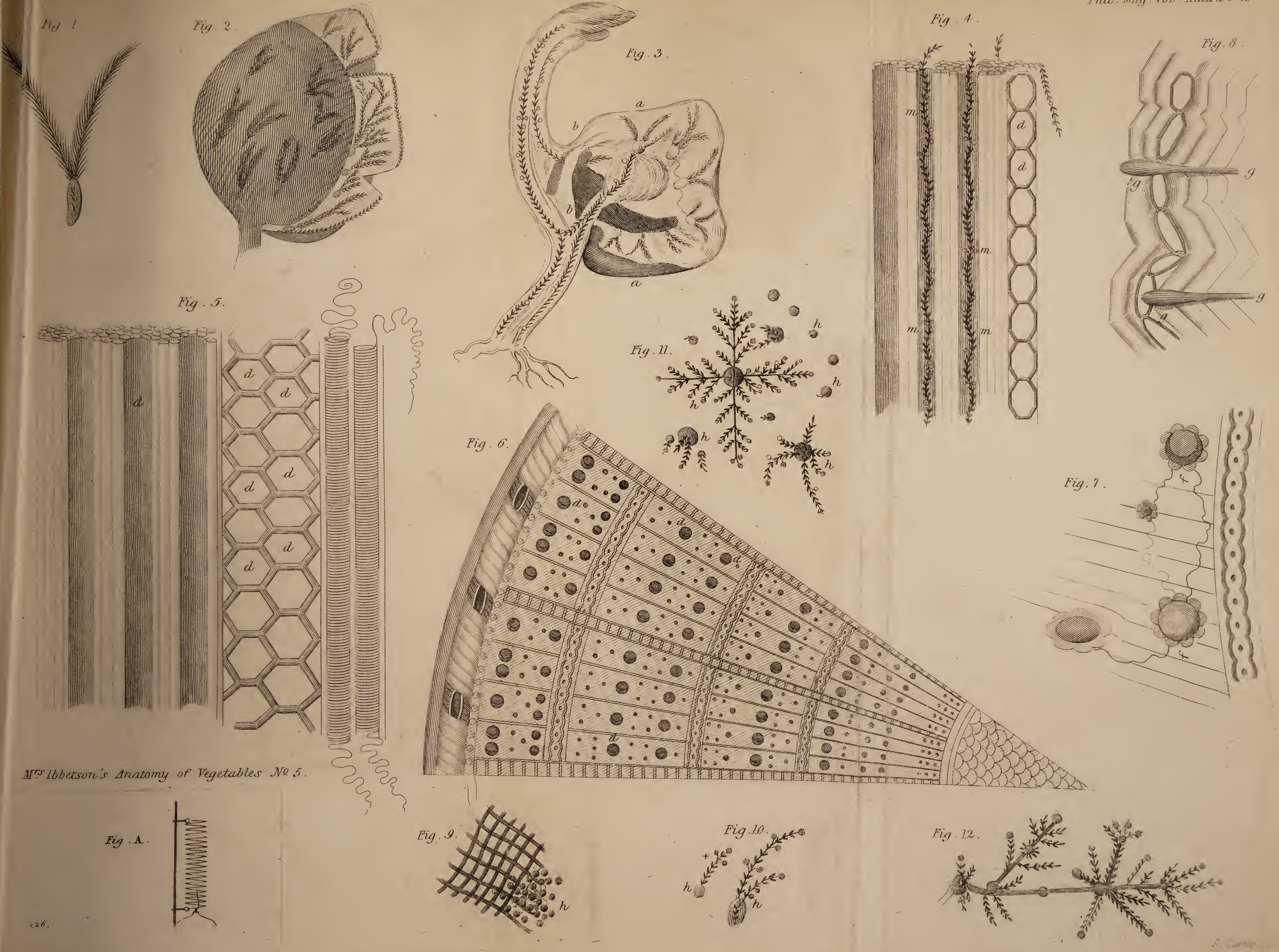


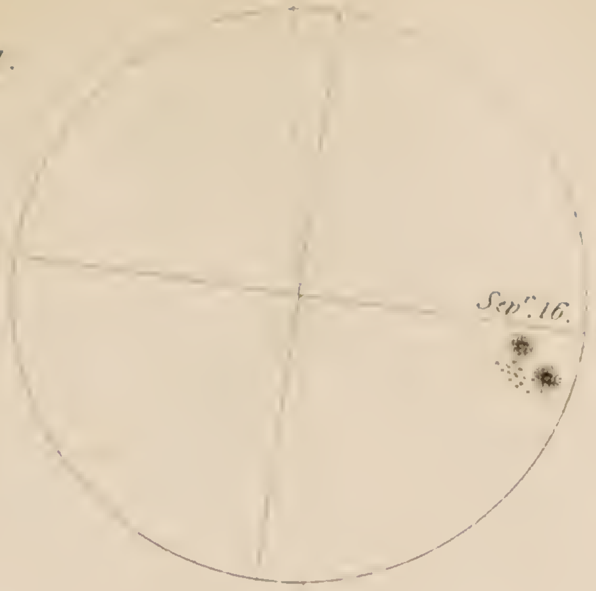
Fig. 1.
E''
E'
N
A
M''
M'
M
Fig. 2.
E'
E
Fig. 3.
M
N
A
E
E'
Fig. 6.
G
C
B
A
F
D
Fig. 7.
H
Fig. 5.
West
East
C
D
E
A
F
G
H
I
B
Fig. 8.
Fig. 9.
North
South
West
Spring Equinox
Summer Solstice

S. Porter



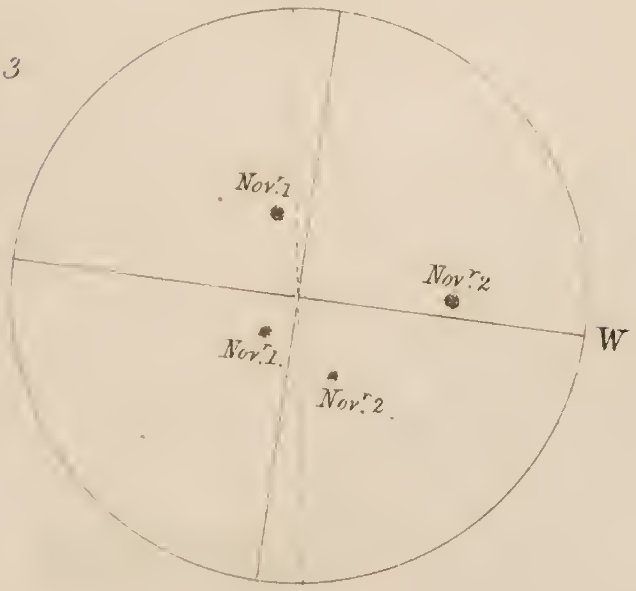
Mrs Ibbetson's Anatomy of Vegetables No 5.

No. 1.

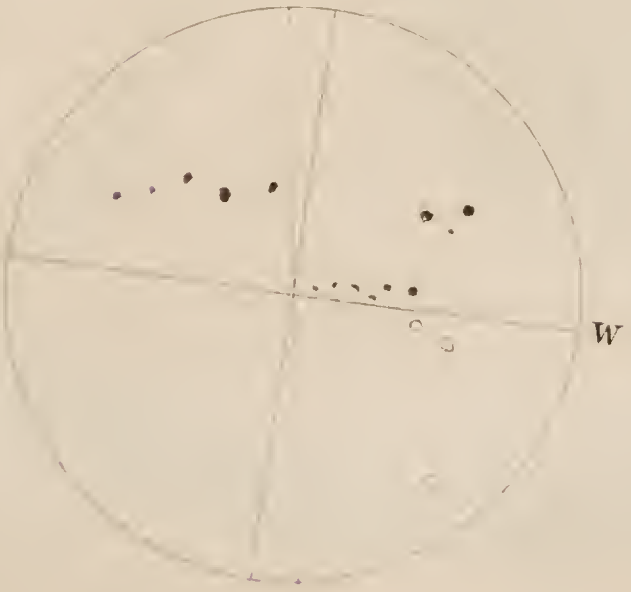


No. 2.

No. 3



No. 4.



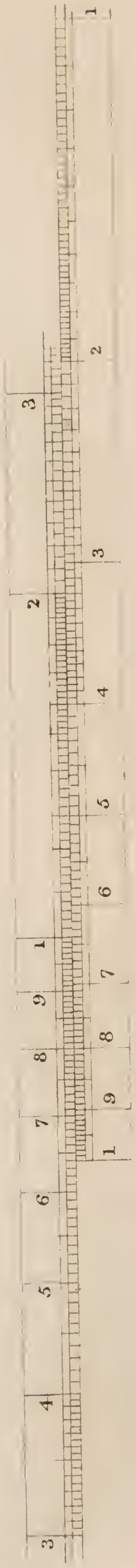


Figure to illustrate a description of an Improved Sliding Rule
by S. Bevan

S. F. sc.

Fig. 1.

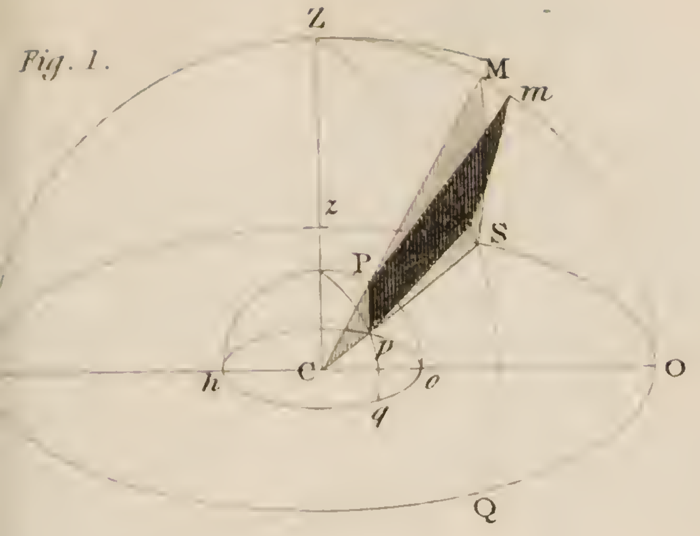


Fig. 3.

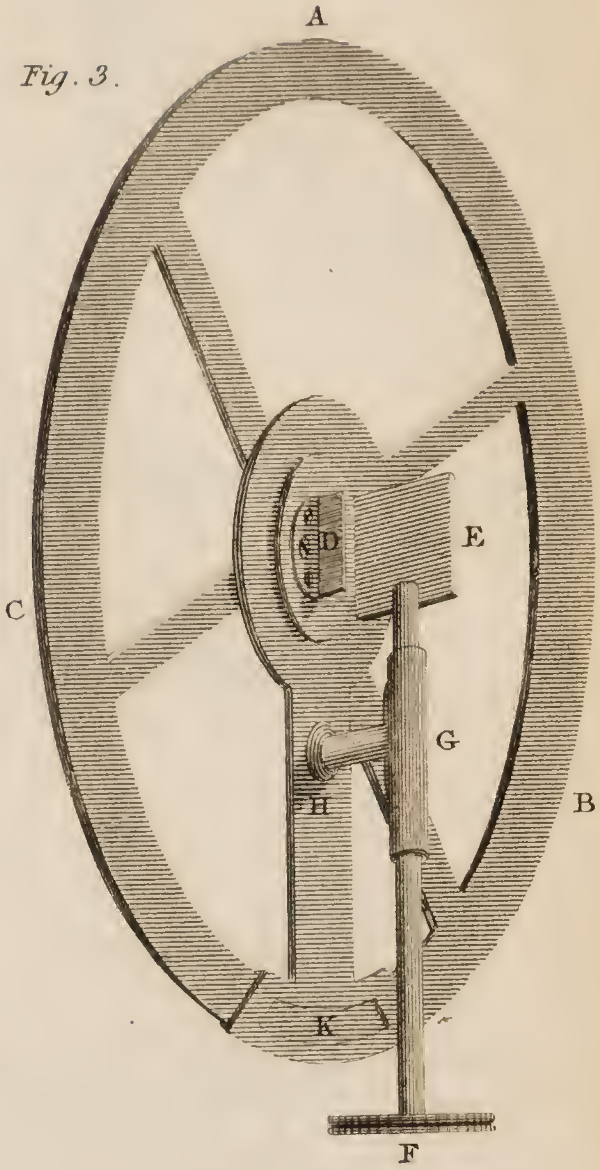


Fig. 4.

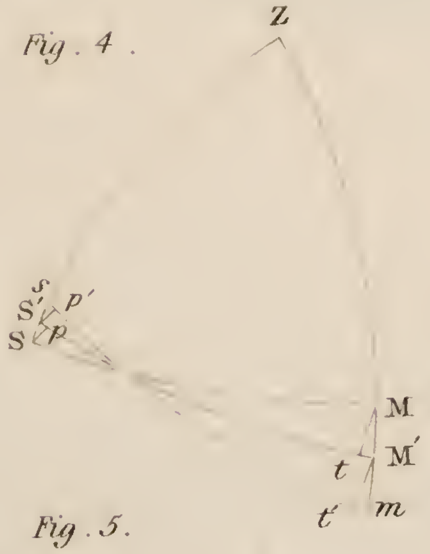
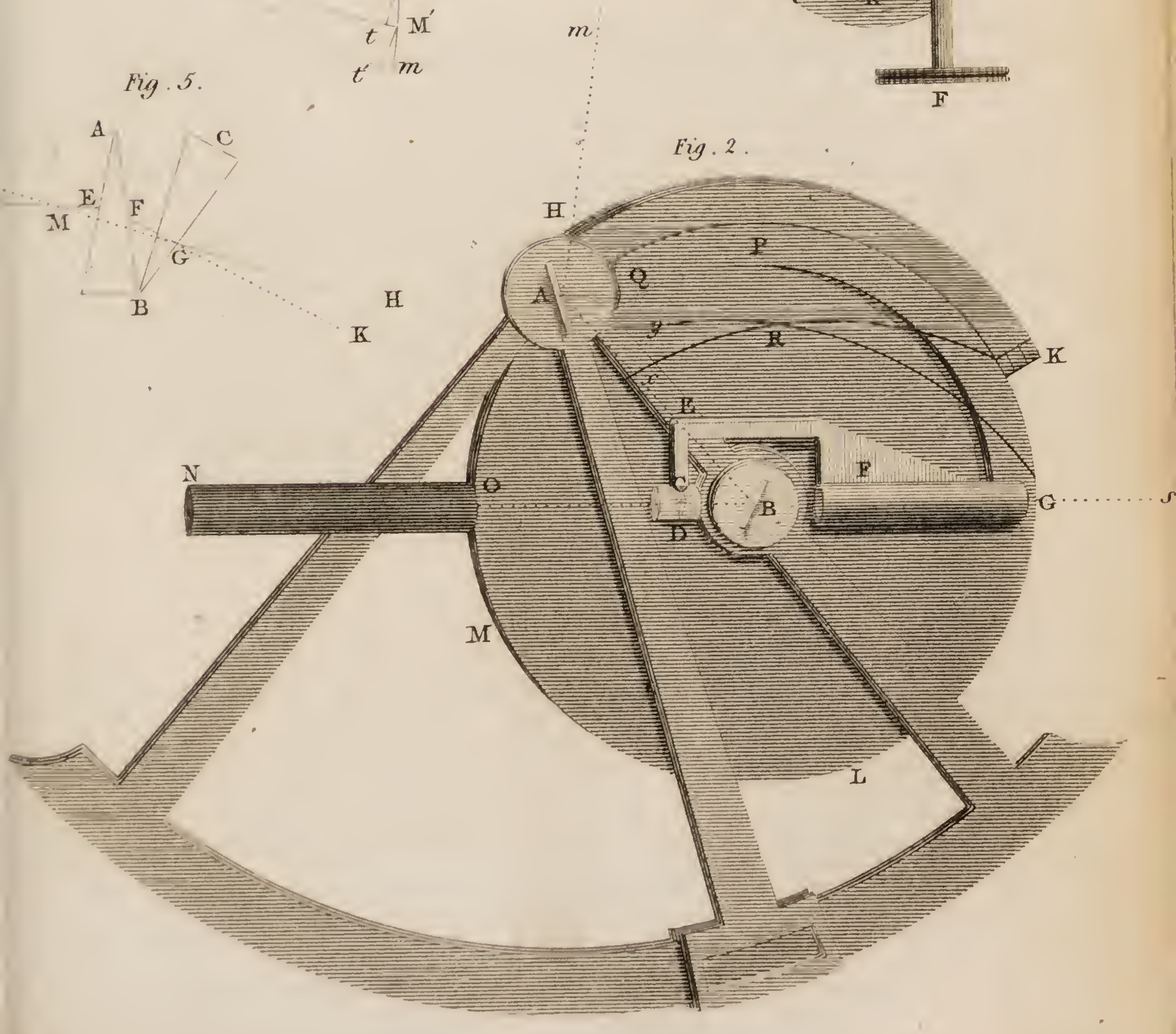


Fig. 5.

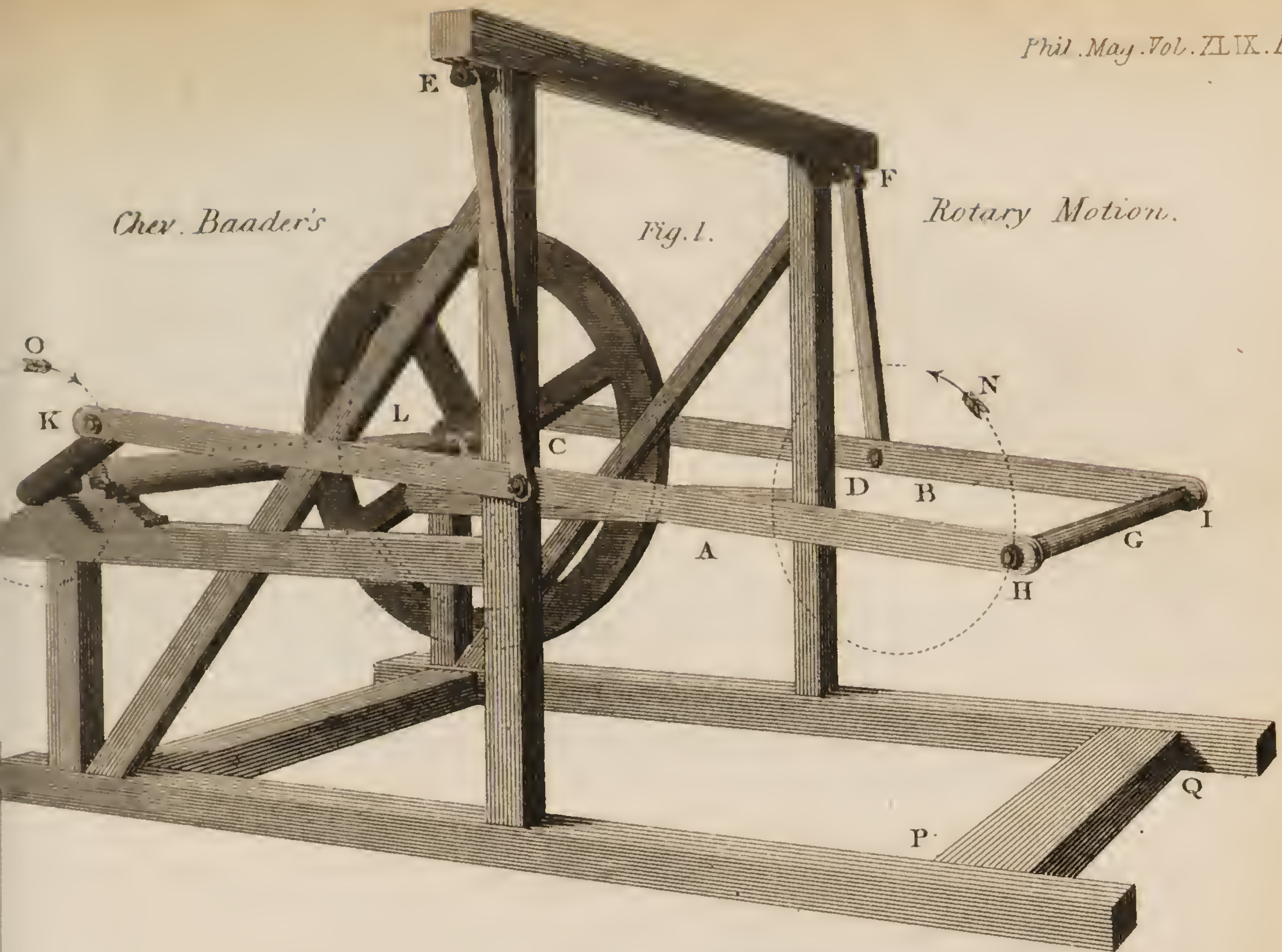
Fig. 2.



S. Porter sc.

Chev. Baader's Rotary Motion.

Fig. 1.



Lieut^t. M. Shuldham's Improved Capstan.

Fig. 2.

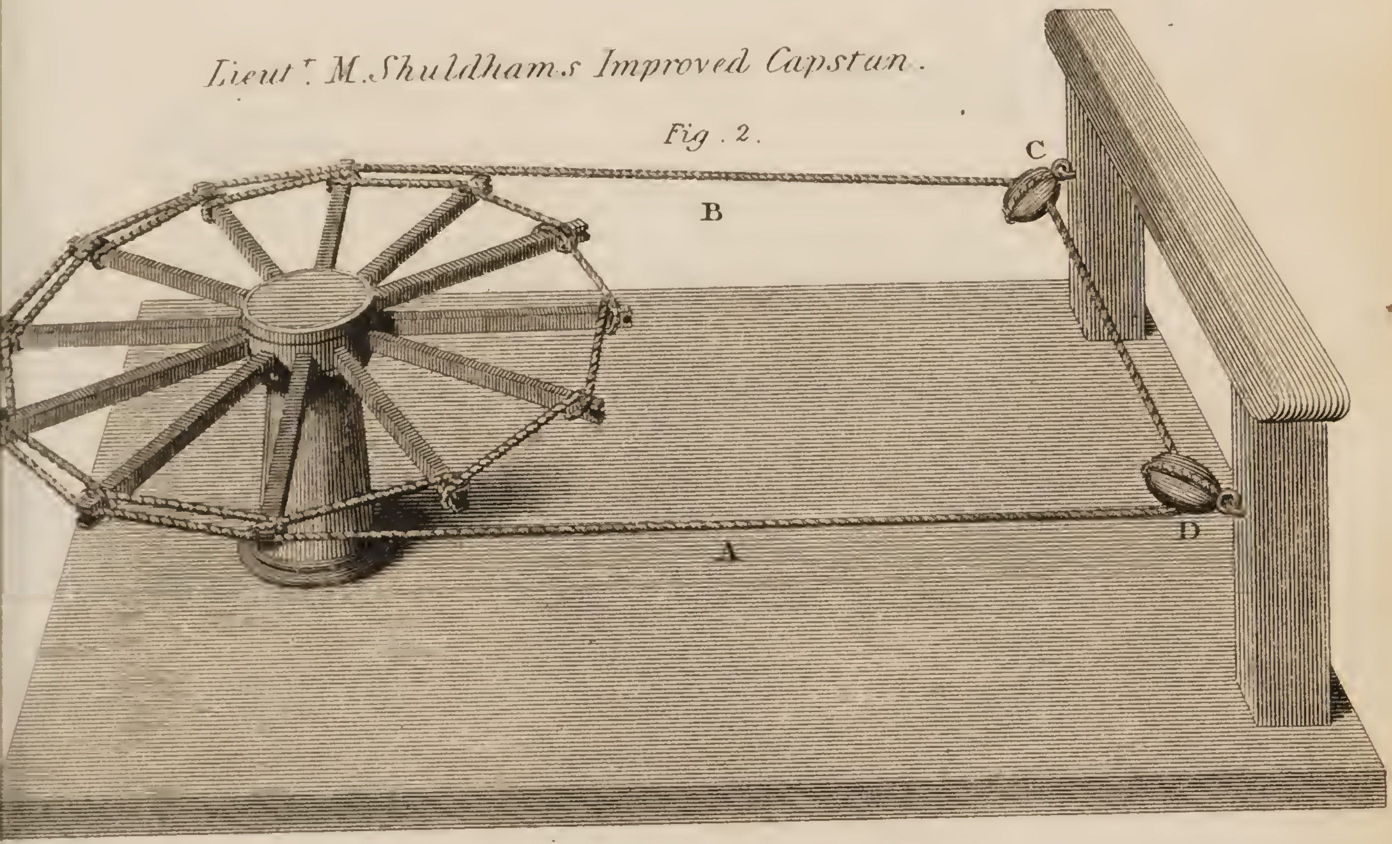


Fig. 3.